

# DISCUSSIONS ON Child Development

The Fourth Meeting of the  
World Health Organization  
Study Group on the  
Psychobiological Development  
of the Child  
Geneva 1956

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EDITORS

J. M. TANNER AND BÄRBEL INHELDER

PREFACE BY PROFESSOR G. R. HARGREAVES

VOLUME IV

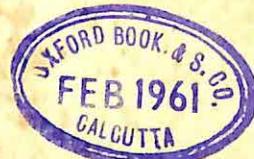
Each year from 1953 to 1956 the World Health Organization brought together some dozen experts to discuss the influences of biological, psychological, and cultural factors in the development through childhood of the adult personality. This group, of international composition, represented a wide range of scientific disciplines: from ethology, Konrad Lorenz; from anthropology, Margaret Mead; from psychology, Jean Piaget, Bärbel Inhelder, and René Zazzo; from psychoanalysis, John Bowlby; from electrophysiology, W. Grey Walter, A. Rémond, Marcel Monnier, and K. A. Melin; and from human biology, J. M. Tanner.

The group, with Dr. G. R. Hargreaves, then Chief of the Mental Health Section of W.H.O., as Secretary, met each year for four years under the chairmanship of Dr. Frank Fremont-Smith, Medical Director of the Josiah Macy, Jr., Foundation of New York.

At each meeting two or three guests were invited to participate. For the fourth and last meeting, reported in this volume, they were Mr. Erik Erikson of Stockbridge, Mass., psychoanalyst and Professor at the University of Pittsburgh, who also attended the third meeting, and Dr. Ludwig von Bertalanffy, biologist and General Systems theoretician from the University of Southern California.

The present volume differs from the three previous ones in being divided into two parts. The second of these is the edited form of the transcript of the discussions of the fourth meeting and thus similar to the material in Vols. I, II, III; the first part contains an essay by Professor Jean Piaget on the chief problems underlying the study of child development, comments made upon this essay by members of the group, and a final reply made after the meeting by Professor Piaget. This essay and the subsequent discussion have given Piaget the opportunity to expound his thoughts on the development of cognitive processes, and particularly logic, in the growing child, with a clarity perhaps greater than ever before. Much of his exposition is couched in the language of cybernetics. In Part Two there is a presentation by Erik Erikson of stages in

*continued on back flap*



DISCUSSIONS ON  
CHILD DEVELOPMENT

VOLUME FOUR

# DISCUSSIONS ON Child Development

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A Consideration of the Biological, Psychological, and  
Cultural Approaches to the Understanding  
of Human Development and Behaviour

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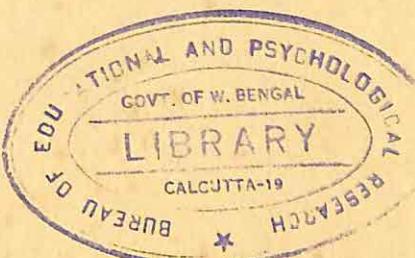
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## VOLUME FOUR

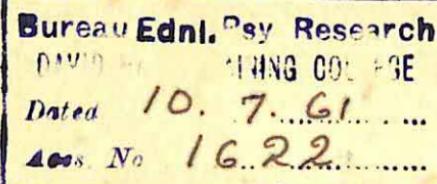
*The Proceedings of the Fourth Meeting of the  
World Health Organization Study Group  
on the Psychobiological Development of the Child  
Geneva 1956*



TAVISTOCK PUBLICATIONS

*First published in 1960  
by Tavistock Publications (1959) Limited  
11, New Fetter Lane, London, E.C.4  
and printed in Great Britain  
in 10 point Times Roman by  
C. Tinling & Co., Ltd., Liverpool, London, and Prescot*

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## PREFACE

This volume gives an account of the fourth and last meeting of the W.H.O. Research Study Group on the Psychobiological Development of the Child and also includes the extensive correspondence which preceded and prepared for the meeting. The editors, as they explain in their foreword, have wisely decided that this preparatory correspondence forms an essential part of the record of the meeting. Hence its inclusion.

It is difficult to write a preface to this volume. Indeed, it should be emphasized for the benefit of the reader that the only adequate preface available to the volume is to read the three volumes which have preceded it. Each of the three previous meetings was devoted to a discussion of data presented to the Group from the point of view of a specific scientific discipline, whereas at this last meeting the data which the Group examined and discussed was its own previous meetings. The account of this examination and discussion can be fully understood only by the reader who has shared, in as far as the printed word permits, the previous shared intellectual development of the members of the Group. But for the student of human development who comes to this volume prepared for it by its predecessors there will be a rich reward regardless of the particular scientific discipline to which the reader belongs.

The meeting was, in effect, a search for a synthesis of all the data that had been presented and discussed at the previous meetings but, as Professor Piaget says in his opening presentation, 'a synthesis which will not be one of doctrine, but which will consist of an arrangement in order of possible questions and explanatory models, so as to delineate the field of interdisciplinary research which will be most usefully followed in our subsequent work'. In its search for a synthesis the Group was aided by a presentation on General System Theory from a new guest—Professor Ludwig von Bertalanffy.

In previous volumes I have acknowledged the Group's indebtedness to many people who contributed to the success of their meetings, but in three directions our indebtedness is so great that it should be re-affirmed. Firstly, to our Chairman—Dr. Frank Fremont-Smith—who enabled this Group to become an effective instrument of scientific exploration; secondly, to our editors, who have so skilfully preserved in written form the essence of our discussions; and finally, to the Director-General of W.H.O., Dr. M. G. Candau, to his predecessor, Dr. Brock Chisholm, and to the late Dr. Norman Begg, W.H.O. Regional Director for Europe.

As the only member of the Group who had the opportunity of seeing, while serving with W.H.O., the extent to which the setting up of the Group and the continuation of its work depended upon the support of these three officials of W.H.O., I feel a strong sense of personal gratitude to them. At a time when the climate of opinion in the governing body of W.H.O. was biased in favour of 'action' and 'quick results' and against 'research' and 'theory', they were among the few who never forgot that the future development of medicine in general, and preventive medicine in particular, depends

upon current research in the present—that the future technology of preventive and clinical medicine will rest upon the findings of pure science. The climate of opinion has now changed and the World Health Organization has now recognized the importance of fostering research. This is due in large part to their leadership. Perhaps also the success of the work of the Research Study Group itself has contributed in some small degree to that change.

*Leeds*

G. R. HARGREAVES

## FOREWORD

A word of editorial explanation concerning the structure (if not the equilibrium!) of this volume may not be amiss, particularly to readers of the previous three volumes, accustomed to *Discussions* opening, as it were, with a bang. Prior to the fourth meeting Professor Jean Piaget was prevailed upon to precirculate to all members and guests of our group a paper containing his views of what were the basic questions to be answered, or rather asked, by all those of whatever discipline who were investigating the development of the child. He ended this paper by addressing a series of specific questions to each member of the Group, as well as a series of general questions to us all. It is a measure of the intellectual weight of Professor Piaget's paper that the replies that came back were prompt, lengthy, and deeply considered. They were in turn precirculated.

The meeting followed with a further thrashing out of the issues raised. Then, at its close, Professor Piaget was asked to draft a short summary statement following on his opening paper, but taking account of our comments and our discussions upon it. Once again, at great pains to himself, he complied.

We have now placed this opening paper, the replies, and the post-discussion paper all as Part I of this volume, and followed it with the usual edited discussion as Part II. We have been in some doubt whether the post-discussion paper should appear where it does, or at the end of this volume after the discussion, and warn readers that they may be best advised to read the precirculated papers first, then the discussion of Part II, and finally this last section of Part I. At least this may be the best course for a first reading; for we make bold to suggest that many readers may feel rewarded by a second journey, one way or another, through some of the papers of Part I.

As this is the last volume, at any rate of the current series, perhaps we may be allowed a few words in a more personal vein. As editors, charged with the task of reducing the original table-talk to an integrated discussion shorn of the ethology of the red herring, we have had more opportunity to view the texts, total and condensed, than any other of our members. We have seen at first hand the growth of mutual comprehension amongst our Group. We have seen many times how remarks identical in content in the first and fourth meetings were on the first occasion passed over in uncomprehending silence by the Group and on the fourth handled easily and accurately as part of the common fund of knowledge. We have watched the gradual and painless assimilation by each member from year to year

of ideas and attitudes at first foreign and, perhaps, even uncongenial to him. We wish to make a sweeping and general claim: that Study Groups of this sort—containing a dozen or so people, each eminent in his own field, but each or nearly each drawn from a different field, meeting at yearly intervals for not less than three years, generating a discussion whose form is determined as much after the event as before it—such Study Groups are the means of education, not at the postgraduate but at the truly professorial level. All of us deplore our increasing specialization; all of us deplore our lack of acquaintance with fields other than our own; all of us deplore the absorption in our own ideas that gradually overtakes us unless we learn continuously as well as teach. The experiment of this international Study Group has, we believe, given us a glimpse of what might become a general pattern for maintaining those precious and precarious possessions, wide horizons and flexible minds.

J. M. TANNER  
BÄRBEL INHELDER

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PART I  
PRECIRCULATED PAPERS

# The General Problems of the Psychobiological Development of the Child<sup>1</sup>

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Having given most of the time at our previous meetings to the study of special problems of child development, we agreed to devote this fourth and last meeting to a discussion of the more general problems, such as the identification of the factors affecting development, of the stages of development and particularly of the mechanisms enabling the transition from one stage to another to be explained.

Each of us has already contributed a large number of data on these different points, but what is now required is a synthesis, harmonizing as far as possible the different viewpoints presented. I shall try to formulate this synthesis in such a way that each member of the Group can add to this preliminary paper, so that in the end a more complete picture can be obtained.

## I. FACTORS AFFECTING DEVELOPMENT

Immediately on approaching this first great problem, it can easily be seen, on re-reading the discussions of the Study Group (*Discussions on Child Development*, Volumes I-III), that we did not keep to the simple and traditional distinction into three main factors of development:

- (a) Hereditary factors, manifested in physical growth and especially in the maturing of the nervous system;
- (b) The action of the physical environment (nutrition and the experience of handling objects), and
- (c) The action of the social environment.

On the contrary, we constantly tried to overcome this dangerous partitioning and, if our respective contributions are carefully examined, it can be seen that we did so in three ways:

<sup>1</sup> This essay, not originally intended for publication, was contributed as a starting-point for the discussions of the fourth meeting, with the particular hope of helping to synthesize the views of the several members.

- (1) by searching for interactions between these three factors;
- (2) by searching for a common language making it possible to describe all three and to formulate their interactions more clearly;
- (3) by recognizing either implicitly or explicitly the existence of a fourth factor, (d), additional to factors (a), (b) and (c), introducing new elements, while at the same time making it possible to co-ordinate them.

### 1. *Search for interaction between factors (a), (b) and (c)*

It might have been expected at first glance that the position taken by the members of the Group with regard to factors affecting development would be determined chiefly by the field in which they had made their own discoveries; for example that Lorenz would explain everything by innate mechanisms and the spontaneous activity of the nervous system; that Margaret Mead would explain everything by social factors and that Zazzo (as a disciple of Wallon) would base everything on the maturing of the nervous system and social factors, while underestimating the importance of the individual's actions in dealing with his experience.

However, the first result of the discussions of the Study Group, the first concrete element of the 'synthesis' which you have asked me to make, is that we are unanimous in considering that the three factors (a), (b) and (c) *never* occur independently of each other and that their *interactions* are consequently at least as important as their respective actions.

Below are some examples of this:

For the discussions on cerebral activity (EEG, etc.) I shall restrict myself to a quotation from Grey Walter (1953):

'The crude division of all human attributes into "inherited" and "acquired" is excusable but quite unreasonable. Even in the simple models of behaviour we have described, it is often quite impossible to decide whether what the model is doing is the result of its design or of its experience. Such a categorization is in fact meaningless when use influences design, and design use.'

As regards Lorenz, I would recall the moment (which appeared decisive for him and myself) at the end of the discussions during the London meeting (see Volume II, p. 260 ff.) where he accepted and stressed my remark that there is no genotype which is not linked to a phenotype, where he discovered with surprise that I was by no means an empiricist (in the sense of explaining development and learning by experience alone) and where he briefly described what he termed his 'dynamic apriorism'. Lorenz's dynamic apriorism, i.e. the concept of an internal activity of the organism developing in constant

interplay with acquired experience, is not very far from development through constant interaction of internal and external factors which we ourselves describe as a continuous formation of structure by successive equilibrations (see Inhelder & Piaget, 1955).

As regards the psychoanalysts who, following Freud's early opinions, explained so much by instinct, I do not need to remind you of Bowlby's flexible and delicately inflected attitude, based on continued interaction between instinctive factors and individual experience as well as interindividual or social relations.

As regards the cultural and social aspect, we may recall how Margaret Mead, who proposed repeating in New Guinea certain of our intelligence tests (conservation, spatial relations, etc.) was in agreement with the theory according to which the stages of reactions to these tests might be the same as regards the order of succession, but might be very different as regards average ages or even the non-attainment of higher levels. And this implies that social factors are constantly interacting with other factors (physical experience, etc.) even in such a sphere as the organization of concepts, which is sometimes interpreted sociologically in a rather exclusive and rigid manner (Durkheim, etc.).

However, all this is self-evident. What is more exciting is to trace how the members of the Study Group, who are unanimous in considering the interactions between facts (a), (b) and (c) to be as important as the factors themselves, endeavoured to co-ordinate their viewpoints and to describe these interactions.

## 2. *Search for a common language*

In order to describe development 'synthetically' and, above all, to make some progress in the explanation of these general mechanisms, it is essential to have a common language. Indeed, without a common language we shall never succeed in analysing the actual interactions between the factors and will always return, despite ourselves, to a description by juxtaposition (or accumulation) of influences.

Let us imagine, for example, that some poor child has been studied by each of us for a month or a year and that we then meet to co-ordinate our results. We would know its brain rhythms, rates of physical growth, family conflicts, relations with its social environment, its reactions to problems of intelligence and to the 25 perceptive laboratory tests which my co-workers have already studied in children, the extent of its vocabulary, its drawings, etc., etc. However, and this is the tragedy of present studies on development, we would be incapable without a common language of achieving anything other than an enormous dossier consisting of a series of small mosaic-like chapters, complete with a concluding essay on the

'personality' of the child (with photographs) linking together with varying degrees of imagination a few facts taken from each of the preceding chapters. We would naturally make films and sound recordings to show how 'alive' all this is, but we would nevertheless continue, in the absence of a common language, each to tell his own separate little story in his own language, without making a real synthesis.

Of course, we have often worked like this during the meetings of the Study Group, but we also did something else and your unfortunate colleague given the task of making this synthesis had the great pleasure to find, on re-reading our reports, that very often we also made an effort to translate from one viewpoint into another and that at certain particularly decisive moments we even glimpsed what might be our common language, or the new language of the future. . . .

I shall start with an example. During the last meeting, Erikson gave us a table of the elementary affective stages, going beyond a narrow Freudian framework, and endeavouring to characterize general forms of behaviour by bipolar links such as 'giving-getting', 'autonomy-shame and doubt', 'initiative-guilt', etc. (see Volume III, p. 168). It is clear that such a table, although it may be immediately usable by all those who have specialized in the affective development of the child, represents to those who have limited their field of study to questions of intelligence or thought only a collection of problems without any solution at present. Each of the criteria employed by Erikson could, naturally, also be applied to the field of learning and the structuration of knowledge. But instead of remaining well-defined, as in Erikson's field, they run the risk of becoming more and more vague the more general they become. Consequently, what we require is not a mere extension, with the risk of increasing inexactitude, but a translation into a common language. While Erikson was speaking, however, Grey Walter was looking for such a translation, of which he gave shortly afterwards a series of examples. Speaking from the viewpoint of 'statistical neuro-physiology' he endeavoured to re-interpret Erikson's stages in the context of information theory and, even if we do not accept this parallelism in detail, we cannot help recognizing the fact that he made use of a much more general language, enabling more precise comparisons to be drawn between the various aspects of behaviour and in particular between its affective and cognitive aspects.

For example, the stage of 'giving-getting' with, as poles, 'trust' and 'mistrust', would correspond to an initial insufficiency of information, such that the elementary exchanges 'giving-getting' are accompanied from the viewpoint of the 'baby-computer' by a degree of approximation large enough to make the system less

precise and consequently more 'trusting'. Similarly, a certain particular type of learning would correspond to the next of Erikson's stages and so on.

The details of such analogies produced on the spur of the moment by Grey Walter are of little importance. Their great significance is to show that one of us who works continually with mechano-physiological models was able to give, in terms of probability of information, an immediate translation rendering the stages of affective development still clearer for those of us concerned with intelligence or with learning.

In fact, this probabilistic language is clearly the common language that we are looking for, provided that the information and communication schemata are supplemented by introducing the concept of 'strategy' and the terminology of the theory of games. In this broadened form the probabilistic language may be suitable for all of us. In the first place, its generality makes it possible to establish fairly direct correspondence between the mechano-physiological models and the various forms of behaviour observed in the psychology of the cognitive functions. In the second place, it is not restricted to describing the information as such, under its cognitive aspect, but, by introducing the concept of gain and loss it provides a means of analysing the 'economics' of forms of behaviour. It is without doubt this 'economics' of forms of behaviour which constitutes the most natural transition between their affective aspect (which can always be translated in terms of enrichment and impoverishment) and their cognitive aspect. In the third place, it enables certain isomorphisms to be found between models of intra-individual operations and inter-individual or social ones and this makes it possible to by-pass the over-simplified and crude antithesis of the individual and the social factors, which is as much a drawback for the theory of development as the distinction between 'innate' and 'acquired'.

### 3. *Recognition of a fourth factor (d) of development*

As soon as we adopt this broader viewpoint, as imposed on us by the search for a common language, we perceive that there exists a fourth factor, more general than the three classic factors of innateness, physical experience and social environment, and obeying its own special laws of probability and the minimum: this is the factor of *equilibrium* which is found associated with each of the three preceding ones, but which governs particularly their interactions and which, moreover, reveals itself frequently in an independent manner.

To give an idea of what such independence may signify, let us take an entirely theoretical example but one which has the advantage of

posing the problem in one of its most general biological forms. We may suppose that in the course of development certain sectors of the organism can be considered as a closed system and are found to obey the second law of thermo-dynamics. In this case the constant increase in entropy, tending towards that state of equilibrium which is maximum entropy, would constitute neither an innate mechanism nor an acquisition in terms of environment, but the result of a purely probabilistic mechanism. We may suppose, on the contrary (like Helmholtz, Guye, etc.) that physical development does not obey the second law. In this case the state of equilibrium towards which growth tends would be characterized by a system of regulations controlling chance; and the overall form of this system would constitute a factor leading to a better understanding of developmental theory than any number of details of various hereditary, acquired or social factors.

To return to concrete problems which are apparently completely different in each of our many fields of investigation, it is very striking to observe how the equilibrium problem constantly recurs, either explicitly or implicitly, in each field which we are studying.

To begin with social factors: even if we accept the great plasticity which Margaret Mead attributes to mental characteristics under the influence of various communities, nevertheless society is not the source of the nervous system, and consequently the many more or less stable reactions which we observe in the different communities constitute more or less complex forms of equilibrium between the psycho-physiological aptitudes of the individual and the actions of the environment. Thus it is not by chance that in the book entitled *Family, Socialization and Interaction Process*, Parsons & Bales (1955) particularly stress states of equilibrium and of disequilibrium, and the double equilibrium peculiar to the internal system of the personality and the system of social exchanges (see in particular a formalized diagram of these equilibrium systems in Appendix B of above, by Morris Zelditch, Jr.).

In the field of affective development it would be particularly interesting to translate social and dynamic psychoanalysis, as understood by Bowlby or Erikson, into the language of equilibrium. It is clear, for example, that the Oedipus stage represents a certain form of affective equilibrium, characterized by a maximization of the 'gains' expected from the mother and by a minimization of the 'losses' expected from the father. In this connexion it would be of interest to examine whether the equilibrium point corresponds merely to a Bayes strategy, the criterion of which would be a simple maximum of 'gain minus loss', or whether it corresponds to a 'minimax' strategy, with a search for the minimum or the maximum loss which the subject supposes a hostile environment is trying to inflict on him. It is evident that a problem such as this cannot be treated in general

since it depends for its solution on the overall environmental conditions for each child.

Besides these problems of 'cross-sectional' equilibrium at any given moment raised by each of the essential phases of affective development, there remains also the essential problem of the equilibrium between the *previous* affective schemata of the subject and the exigencies of the *present* position.

From the mechano-physiological viewpoint, the part played by the concept of equilibrium, and especially progressive equilibrium, is particularly important. This is because of the perspectives it opens up not only as regards the process of problem-solving and of what Ashby calls the 'finalized mechanisms', but also as regards the general lines of development of the cognitive functions. An apparatus which solves problems by a succession of approximations based on a series of feedbacks shows in the most decisive manner the part played by the concepts of disequilibrium and of progressive equilibration. As long as there is disequilibrium, i.e. while the problem still remains unsolved, a new negative feedback is set off, whereas the attainment of the correct solution is marked by the production of a state of equilibrium. Furthermore, successive approximations to the solution correspond to a progressive equilibration in accordance with a series of steps. These steps can be thought of as corresponding to phases in the processes of adult problem-solving ('Aktualgenese'), or even to stages in the developmental capacity of the child.

It is in fact very suggestive to compare such an equilibration mechanism with the processes of solution of a conservation problem in the child, since this last class of facts is of such a nature as to show the fundamental part played by the concept of equilibrium, not only in the mechanism of solution of problems but also in the development of cognitive functions in general.

From the first of these two viewpoints, if we study a child aged seven or eight years who begins by denying that there is conservation of matter when a ball of clay is moulded into a progressively longer and longer sausage and then discovers during the actual experiment the need for such conservation, we can distinguish the following phases:

1. During an initial phase, the child perceives perfectly well the lengthening and gradual thinning of the sausage, but he chooses the simplest strategy and reasons only on one of these two properties: he will say, for example, that the sausage in state A contains **more modelling clay than the ball because it is 'longer'** and that the **sausage in state B contains still more because it is 'still longer'**, etc.
2. During the second phase, the error is corrected by its very exaggeration (negative feedback): when the sausage has become too long (state C or D) its thinning, which up till then was forced into the

background, reappears in the foreground by a kind of backward step or regulation and the child says: 'Now there is less clay because it is too thin.'

3. During the third phase, there is a sudden change in strategy and an arrival at the equilibrium point: instead of reasoning as before on the properties of the states ('longer' or 'thinner') the child begins to reason about the transformation itself: the ball is drawn out, consequently it is lengthened and made thinner at *the same time*, thus one of the two changes compensates for the other, consequently there is *conservation*.

This small example reveals the development of the cognitive functions as a whole, since equilibration plays the part there of a fourth fundamental factor of evolution. Indeed, the clearest result of our researches on the intelligence of the child is that intelligence in course of formation is oriented in the sense of a progressive *reversibility*. Thus the act of intelligence consists in grouping or co-ordinating operations: however, operations are actions which are interiorized and have come reversible, like addition which is derived from the action of bringing together and which can be reversed in the form of subtraction. On considering the evolution of operational systems, one finds three stages, corresponding in outline to the phases of the 'Aktualgenese' in adult problem solving.

1. During an initial stage, which marks the commencement of early childhood, we find rhythmic-activity actions tending towards a material aim or success are only uni-directional or irreversible. It is because of this irreversibility that the child lacks the notion of the persistence of material objects.
2. During a second stage, this initial irreversibility, which characterizes intelligence or thought in course of formation as well as the most general cognitive functions (from perception to habits, associations and memory) is tempered by a more and more complex system of regulations, which constitute a state of semi-reversibility.
3. At a third stage there is the development of operational structures which are characterized by their strict reversibility. The most direct result of these operational mechanisms is then the formation of concepts of conservation: the invariants or conservations (of geometric or physical properties of objects, or of whole or discontinuous quantities, etc.) always, in fact, appear as the product of a particular form of operational reversibility.

This reversibility, which may usually be considered as the most specific characteristic of intelligence, is nothing other than an expression of a law of equilibrium. Whatever the relative contributions made to the formation of reversible operational mechanisms

by the maturing of nervous co-ordinations, by physical experience, and by social relations, the principal property of such mechanisms is that they are systems which are both mobile and stable, characterized by virtual transformation and by exact compensation. This makes it possible to conceive of the development of intelligence as being directed towards various forms of equilibrium.

To summarize: it is found that if, in order to describe the classical factors of development, one adopts a common language consisting of the modern applications of probabilistic language (theory of information, games theory), then one is forced to recognize the existence of a fourth factor of development, a factor of equilibration. Moreover, it is seen that this fourth factor is common to all our respective fields of investigation, since it is found in the social field, the affective field, in the mechano-physiological realm and in the sphere of the cognitive functions.

## II. THE PROBLEM OF THE STAGES OF DEVELOPMENT

Although we are specialists on development, we still have not found out whether we understand the concept of stages in development in the same way and whether we can hope some day to establish some relationship between our respective stages! Indeed it is not clear whether all of us would subscribe even to the existence of stages. Tanner particularly stresses in his field the continuity of physical growth. Nevertheless, this problem was the starting point of our first meeting; thus it would seem essential to revert to it in the final synthesis.

The first problem confronting us is that of the actual concept of stages, regarding which our respective positions diverge rather considerably. Certain schools, for example, limit the characterization of the stages to a consideration of 'dominant characteristics'. Thus, Freud speaks of an oral stage at a period where the child already makes use of his anus and of an anal stage at a period where he still makes use of his mouth. Similarly, Wallon, represented in our Group by Zazzo, speaks of an emotional stage at a time where the infant is already exercising all kinds of sensorimotor functions and of a subsequent sensorimotor stage during which emotions are by no means absent.

Others among us demand more complex criteria. For example, Inhelder and I, when considering the development of structures and of thought, speak of stages only in connexion with the formation of total structures. We include as special cases all structures observable during a given stage which integrate with the structures of the preceding stage as necessary sub-structures. In this way the logical

operations of the 'stage of formal operations' (from 11-12 to 14-15 years) constitute a total structure whose two complementary aspects are the formation of a 'lattice' (combinatory aspect) and the constitution of a 'group' of four transformations (double reversibility). However, this general structure covers, on the one hand, all the operational schemata of this stage (combinatory operations, proportions, double systems of reference, etc.) and, on the other hand, implies as sub-structures the general structures of the preceding stages (in particular the characteristic 'groupments' of the 'stage of concrete operations' from 7-8 to 11-12 years: classifications, seriations and correspondences). If we wish to aim at a synthesis in the fundamental problem of stages, we must first agree on the criteria of the stages. We have just indicated two possible criteria (dominant characteristic or total structure) but there may be many others. However, even if we limit ourselves to these two possible criteria the problems which they raise are immediately visible.

1. If we restrict ourselves to dominant characteristics, then by what objective signs can we recognize a characteristic as really dominant? Can we hope to furnish statistical criteria of frequency, or must we be content with a clinical impression, which runs the risk of being subjective? Does dominance imply a tendency towards integration of the other characteristics under the dominant characteristic (which would bring us close to the concept of 'total structures') or shall dominance be defined only in terms of relative importance from the viewpoint of frequency?

2. If the requirement of general structures is imposed, then what would be the field of application of such structures, and, in the case of several distinct fields which one wishes to interconnect, what language should be used to describe these structures? Inhelder and I have restricted ourselves to the study of intelligence and of thought and in this field—perhaps a special one as regards stages—the concept of total structure takes on a precise sense which can be defined in terms of general algebra and symbolic logic. But these concepts no longer apply when we come to perception and we have not found stages of the development of perception, at least in a form as simple and clear as our stages of the development of intellectual operations. Can we, then, hope to apply the criteria of total structure to the stages of social development, affective development, or psychomotor development, and, if the answer is 'yes', in what language should they be expressed, since affectivity, for example, characterizes the energy component of behaviour rather than its structure?

It can be seen that this initial problem in the delimitation of stages brings us back to questions very close to that of the part played by equilibrium discussed above. To the extent that objectively certain stages exist (and this is indisputable in certain fields), they cannot

be considered as a product of subjective cuts arbitrarily made by the research worker in a rigorously continuous development. If stages do exist objectively, they can only consist of successive *steps* or *levels* of equilibrium, separated by a phase of transition or crisis, and each characterized by a momentary stability. The criteria employed to characterize the stages would then reduce to criteria of equilibrium: the 'total structures' are 'equilibrium forms' and the 'dominant characteristics' are linked to a certain property of equilibrium, existing at least momentarily. Generally speaking it would thus be again the language of equilibrium, which would be most suitable for reaching co-ordination between our different viewpoints on this problem.

Consequently, it is essential for us to begin by establishing a series of criteria of what we call stages (see Inhelder, First Meeting, Vol. I, p. 84) by proceeding systematically from what could be termed a *minimum* programme to a *maximum* one:

1. The *minimum* programme for establishment of stages is the recognition of a distinct chronology, in the sense of a *constant order of succession*. The average age for the appearance of a stage may vary greatly from one physical or social environment to another: for example, if the children of New Guinea, studied by Margaret Mead, manage to understand, like those of Geneva, certain structures of Euclidian geometry, they may do so at a much later or much earlier age. Whether older or younger is of little importance, but one could not speak of stage in this connexion, unless in all environments the Euclidian structures were established *after* and not before the topological structures considered as primitive.
2. A further step is taken in establishing a programme of stages when one succeeds in finding the equivalent of an *integration* in the transition from a lower stage to a higher one. As regards intellectual operations it is clear, for example, that the initial sensorimotor structures are integrated into the structures of concrete operations and the latter into formal structures. But can one say as much of the classic Freudian stages, and is it possible to agree that the elements of oral and anal stages are integrated at the level of the Oedipus stage? The great merit of Erikson's stages (Vol. III, p. 168) is precisely that he attempted, by situating the Freudian mechanisms within more general types of conduct (walking, exploring, etc.), to postulate continual integration of previous acquisitions at subsequent levels, and it seems to me that Bowlby, while laying greater stress on the essential reality of conflicts, both internal and external, is nevertheless in search of an ideal not very far from such integration.
3. The integration of the elements of a stage  $n$  into the achievements of stage  $n+1$  gives rise to the supposition that if the stage  $n+1$  is really new with respect to stage  $n$ , then in any stage  $n$  it should be

possible to distinguish an aspect of *achievement* with respect to the stages going before and also an aspect of *preparation* with respect to the stages coming after. Naturally it is possible for both achievement and preparation to be promoted or hampered by favourable or unfavourable external situations (hence the possibility of crises as natural transitions between one stage and the next).

4. We advance further towards the *maximum* programme of criteria of stages if we then say that it is justifiable to ascribe all the preparations leading to a stage and all the achievements characterizing this stage, to the existence of a *general* (or total) *structure* in the sense defined above.

5. However, as the concept of structure is perhaps peculiar to certain aspects of development, particularly cognitive functions, and as the corresponding affective aspect is ascribable more to an energy principle than a structure, the most general and the most elaborate programme for a theory of stages doubtless consists in representing the stages in the form of a series of equilibrium levels, the fields of which would be always more and more extensive and the mobility always greater, but whose increasing stability would depend precisely on the degree of integration and of structuration just discussed.

The first problem in our synthesis of stages would consequently consist in deciding whether we accept such a programme and if not, why not, and in the event of our accepting it, which of the five aims of this programme we believe it possible to reach at present in the different disciplines which we represent. To start with it would be a considerable advance in the study of development if we could agree on the actual concept of stages. At present almost all authors interpret this concept quite differently, as was revealed, for example, in the examination of this problem by the third meeting of the *Association de Psychologie scientifique de langue française*, at Geneva in 1955 (*Association, 1956*).

Next, however, comes the second question, which is much more serious: to what extent can we establish co-ordinations, not only between our concepts of stages, but also between our stages themselves? This leads to a still more fundamental problem: do *general* stages exist, i.e. stages including at the same time, for a given level, the totality of the organic, mental and social aspects of development? I would like to submit to the Study Group the following hypotheses, which seem to be the most cautious expression of the degree of synthesis which we may hope to attain.

1. There are no general stages. Just as, in connexion with physical growth, Tanner showed us that there was an absence of close relationship between the skeletal age, the dental age, etc., similarly, in the various neurological, mental and social fields, we see an intermingling

of processes of development which are evidently interrelated, but to different extents or according to multiple temporal rhythms, there being no reason why these processes should constitute a unique structural whole at each level.

2. The unity of the 'personality' is a functional unity, i.e. a unity which is in search of itself and builds itself step by step, but for which it has never been possible to give an adequate and verifiable structural expression; consequently we cannot take such a concept, which conceals an indefinite number of interacting biological, mental and social factors, as a starting point for postulating the existence of general stages.

3. On the other hand, to the extent that the unity of the body and the personality is built up by successive equilibrium levels and through an innumerable series of disequilibria and re-equilibria, it is possible, by following the various developmental series which each of us is studying, to establish groups of *particular* convergences all the more instructive in that they will be better delimited and more advanced in their detailed analysis.

4. It is not only in the correspondence of the stages, or perhaps not even in such correspondence, that one can hope to find the convergences sought for. It is, perhaps, rather in the mechanism of the transition from one stage to the following, i.e. in certain characteristic processes of the actual mechanism of development.

### III. THE PROBLEM OF THE TRANSITION FROM ONE STAGE TO THE FOLLOWING: THE MECHANISM OF DEVELOPMENT

In this part we shall proceed by discussing a few examples:

1. *The construction of the sensorimotor object and 'objectal' relations*  
As I showed long ago, the infant begins by not believing in the permanence of objects when they leave his field of perception. He does not look for a toy which he was going to grasp when it is covered with a cloth. When, at feeding time, I hide his feeding bottle behind my arm a few centimetres from his hand, he screams (even at seven months) instead of trying to grasp it or instead of trying to see it behind my arm, by bending slightly forward (whereas he immediately grasps it if an end of the object still remains visible). Towards the end of the first year, on the other hand, he looks for objects which have disappeared and finds them in accordance with their successive displacements (Piaget, 1950).

In the field of psychoanalysis (with which our researches have had no direct connexion) Freud described how the infant, at first interested only in his bodily functions, ends by objectivizing his

affectivity on persons, and Spitz has devoted a recent series of studies to these 'objectal' affective relations.

In such cases it can be seen straight away how we are faced with the question of the correspondence of stages. At almost the same ages and in both fields one witnesses a parallel transition from an initial state of centration on the subject's own activities (reality is reduced to a dependence on perceptual pictures as related to the momentary actions of the subject) to a final state of decentration wherein the subject becomes conscious of his subjectivity, 'places himself' with respect to a world of external objects and persons. The problems are, then, to know whether one will come across the same intermediate stages at the same ages, or whether the affective stages precede the cognitive stages, or vice versa. On this will be based hypotheses on the interaction, or rather the indissociable complementarity, of the affective and the cognitive, or on the primitive and causal character of one of these two factors with respect to the other. I personally believe in the indissociable complementarity of the cognitive structuration and the affective energy principle, but this is a personal opinion only.

A still more interesting problem is that of the mechanism of the transition from one stage to another (even if the two series of stages do not exactly correspond) for this is a fine example of those general problems of strategy and equilibrium which were discussed in Part I.

A. From the *cognitive viewpoint* the problem is to explain by what strategy a baby, who begins by reducing everything to itself, and by not comprehending the existence of changes outside its own actions, eventually succeeds in objectivizing these changes and attributing them to causal sources independent of itself. It is clear that this discovery is not innate since it results from a long construction. Neither is it due to the teaching of experience alone, since experience itself, which cannot even contradict a radical solipsism, is quite inadequate to correct this kind of egocentric perspective linked to the absence of consciousness of the self which characterizes the initial actions of the baby. This discovery is due to a decentration or inversion of the sense of cognitive perspective, i.e. to a new structuration or to a general equilibration of the spatial, temporal and causal relationships involved, and not to an acquisition based solely on experience. In fact, this equilibration is produced fairly clearly by a combined mechanism of least action and sequential probability:

(a) In the first place, the strategies of the infant may be classified in accordance with an order which is simultaneously chronological and of increasing complexity: (i) the infant localizes the object only in its perceptual field without paying attention to its movements or to

its localization at the moment it disappears; (ii) the infant begins to look for it after its disappearance, but in relation solely to previously successful actions (consisting in finding it in a given place) and not in relation to the successive displacement (although visible) of the object itself; (iii) the infant looks for it in relation to visible and successive displacements, co-ordinating the latter according to a group structure, etc. (The next step consists of an extension to certain invisible displacements.)

(b) It is next found that these strategies are more and more costly to construct but give more and more remunerative results: to be concerned only with perceptual localizations is very simple, consequently costs little, but is profitless, since the object which had disappeared becomes non-existent and is no longer to be located; to localize in relation to previous successful actions is already more costly, since this calls for more complex co-ordinations, but the result is a little more fruitful, although not always. To localize in relation to a 'group of displacements' is much more costly, since this consists in *adding* to the direct perceptual experience new relationships of a temporal nature, spatial references, etc., but the result is much more remunerative, since it makes it possible to foresee the successive positions of moving objects according to a system of localizations and reversible displacements.

(c) Consequently, equilibrium is attained when reality is understood in relation to a system of *minimum* changes (a system of displacements instead of the initial system of continual creations and annihilations) but supplying the *maximum* information (on utilizable objective relationships).

(d) From the viewpoint of strategies, equilibrium is consequently attained with the strategy furnishing the *maximum* of 'gains minus losses', which was the most costly to construct but which has become the simplest to apply (= *minimum losses*) and which gives the *maximum* results.

(e) From the viewpoint of probabilities, one may consider each new strategy as being the most probable, once the results of the preceding one have been obtained (and once the inadequacy and indeterminate-ness of the information to which it led has been observed). The final equilibrium is consequently not the most probable product *a priori* (at the outset), but is the end result of a series of reorganiza-tions, each of which is the most probable one after observation of the failure of the preceding ones (a series of feedbacks finally culminating in stable equilibrium).

B. *From the affective viewpoint* we believe, despite the surprising and paradoxical nature of this opinion, that the procedure followed is not very different! In short, it is not entirely unreasonable to believe

that the fact of maintaining 'objectal' relations with the persons about him is, for the infant, a much more costly strategy, although much more remunerative in affective values of all kinds, than to be content with merely giving play to his sucking reflexes or even his sphincters. What is required is once more to analyse, in terms of *minimum* and of sequential probability, the successive strategies leading from one of these extremes to the other.

We should note, however, that an explanation in terms of profit and loss of the phases of affective development does not signify that the subject (the infant) has himself made a calculation of his interests in each situation lived through. This calculation is made solely in the sense that the fact of experiencing positive or negative *values* (an affect consists essentially in attaching value to a given action) amounts precisely to enriching or impoverishing oneself, from the viewpoint of functional exchanges with surrounding persons. P. Janet has already reduced the elementary feelings (joy and sadness, effort and fatigue, etc.) to the rules of an internal economy of action. This point can be extended to form a theory of values considered as the external economy of the action, and to deduce from it a theory of affective equilibration. From this point of view, the achievement of objectal relationships as enrichment, but also as a more costly strategy, probably marks the arrival at a certain level of equilibrium, which is, however, at the same time a point of departure for numerous and profound disequilibria.

## 2. *The problem of affective schemata (in particular of the superego) and of representative and operational schemata*

A series of convergences between the work of Bowlby and that of the Jean-Jacques Rousseau Institute at Geneva seemed possible during our last meeting and I should like to indicate in a few words how the second example could be developed.

Despite the absence of all direct relationship between our work and psychoanalysis, there exists a certain similarity in the way in which we pose our problems. This common point of departure consists in accepting that all feelings and knowledge have a history and consequently in considering that no external influence represents an entirely new beginning, but is always *assimilated* to all that has gone before, and may modify the subsequent course of the history by giving it an impulse in a partly new direction. The problem then is to understand how this assimilation takes place and in what form the organization of the old and new factors exists.

To this problem, Freud replied that, in the affective field, we retain in our subconscious all our experiences of the past, particularly infantile or early conflicts, and that our subsequent affective life

always consists, to some extent, in the identification of new situations with previous ones, by a kind of fixation of initial images and complexes.

The history of the individual development of knowledge, on the other hand, gives a rather different picture. In general it is not the memories as such of things we know which are retained but rather schemata of actions or operations derived one from the other. These have a constant adaptation to the present and a structuration which is continuous (or in steps), orientated in the direction of equilibration.

However, during a very instructive discussion on the search for common mechanisms of development, Bowlby seemed to admit, if I am not mistaken, that the history of affective schemata and of conflicts was not so far distant from such continuous structuration. He appeared to feel that there is dynamic assimilation rather than strict identification, and that such assimilation proceeds by analogies and transfers and not by any exclusive fixation, in the course of adapting a perpetual reconstruction of the past to the present in various conflictual or equilibrated ways.

The problem of choosing between these two interpretations arises notably in regard to the superego. This may be conceived of either as a simple fixation on past images and imperatives or else as a group of schemata of affective reactions presenting the same factor of continuity in the presence of each new situation but with a progressive flexibility of accommodation in regard to the particular data presented.

It is clear that this problem lies at the root of the question of convergences or of divergences between the affective and cognitive forms of equilibration. One must be very careful, of course, in using a term such as 'equilibrium' which is too convenient and so often merely verbal or literary. The strict rule should be followed of using it only in situations where one has objective criteria such as the indices of *minimum* (including the *minimax*) or of fairly convincing probability schemata. However, it seems impossible to express affective conflict situations in this way, and especially, the various modes of solution of the conflicts, without in the end adopting a rather precise terminology using the language of equilibrium. Even in a conflictual situation where one is losing all round, as in the case of an individual whose superego prevents all adaptation and whose liberation from his superego would represent, moreover, a definite privation, one might still ask whether such an individual would not finish by choosing, out of all these possible losses, the solution consisting in minimizing the *maximum* loss inflicted on him by his history and environment; and, this would constitute a point of equilibrium according to the *minimax* strategy.

Consequently we feel that a discussion on these basic problems would lead to appreciable progress in our projects of synthesis in the interpretation of development, according to whether there is a possible convergence or a necessary divergence between the processes of the history of affects and those of the history of intellectual operations and representations.

### *3. The forms of social interaction and the development of the child*

Let us agree to distinguish in social life between molar phenomena (general form of society and transmission of community culture from one generation to the next) and molecular phenomena (interaction between individuals on various levels). It is evident that there are all kinds of transitions between the molar and the molecular and that the general molar forms influence to a high degree the molecular interactions. However, this very fact makes it all the more interesting to consider whether among the modes of molecular interactions there exist certain tendencies towards equilibrium and, if so, what their relationships to mental development may be.

In European societies such as the one in which we live, it is very interesting to follow up step by step the spontaneous forms of collaboration between children in well-defined situations, such as a constructional game, which can remain individual with various imitations, or become collective to different degrees. Mlle Inhelder, who has recently made such studies, with the collaboration of G. Noelting, will be able to give us more details; for the time being I will simply point out the fact that there is a remarkable convergence between the stages of this social collaboration and that of the formation of intellectual operations, to such a point that one has the impression that there are here two complementary and inseparable aspects of the same process of equilibration.

To cite only one example: the social relation of reciprocity, which gradually imposes itself as a form of equilibrium between individuals considering themselves as equals, assuredly corresponds to the logical and operational transformation of reciprocity which dominates the logic of symmetrical relationships ( $a=b$  therefore  $b=a$ ) and certain equations in the logic of propositions ( $p \circ q = \bar{q} \circ \bar{p}$  and  $\bar{p} \circ \bar{q} = q \circ p$ ). Developmentally speaking the progressive organization of inter-individual reciprocities and that of operational reciprocities in the field of thought certainly constitutes two correlated phenomena, without mentioning moral reciprocity which is of importance in the organization of normative values and which is, in the opinion of all authors, simultaneously social and relative to 'practical thought'.

This being so, a clearly circumscribed problem of possible synthesis between the researches of cultural anthropology and those of child

psychology would consist in determining up to what point a molecular tendency to reciprocity is found, considered as a most probable form of equilibrium between equal individuals, ensuring the *maximum* of performance compatible with the *minimum* of change, and this whatever the molar form of the situations in question. Margaret Mead has described social situations in which a baby sucks anything except his thumb, others in which he does not smile because he passes his existence on his mother's back, without seeing her face, etc. But are there societies without any reciprocity? Sociologists have described primitive forms of exchange, finding them in gifts or presents which sooner or later give rise to reciprocal reactions. These are institutional reciprocities which have become molar. What are the transitions between these molar reciprocities and the many possible forms of molecular reciprocity?

#### *4. Perceptual activities, intellectual operations, and the EEG*

My role in drawing up this draft for discussions directed towards the synthesis of our results is certainly not to appear to have an extensive competence in all our fields of study, but on the contrary to provoke the reactions of each specialist by simply imagining problems of general interest capable of linking together the fields which he knows and those with which he is not familiar. In other words, Part III of this paper, devoted to problems of the mechanism of development, brings us quite naturally to Part IV, devoted merely to a listing of the problems to be discussed, as the author leaves his own field further and further behind and approaches what for him appear merely to be 'promised lands'.

This is the spirit in which I would like to conclude Part III, by describing with some degree of naïvety what I would expect from a theory of the change in the EEG with age if I wanted it to link up with the problems of equilibrium which seem to me most general and most 'synthesizing' in relation to the mechanism of development.

On considering in their most general form the equilibrium of patterns of behaviour (it being understood that the affective factors corresponding to the energy principle of such forms of behaviour and the cognitive factors corresponding to their structure are always complementary and indissociable) I would say that such forms of equilibrium comprise three types:

1. A progressive extension of the field of equilibrium, i.e. of the objects to which the forms of behaviour apply. This extension of the field may be expressed in terms of the 'probability of encounter'. We have endeavoured to give a very simple mathematical model of this probabilistic mechanism in the case of perceptual centrations and in the explanation of elementary perceptual illusions, but the process of 'probabilities of encounters' may be generalized on all

levels, on the understanding that 'encounters' on the higher levels may themselves be functions of a continually more complex schema dependent on the mechanisms which follow.

2. An increasing mobility of the equilibrium, since the equilibrium of behaviours is an equilibrium between actions and movement; actions may be described in their most general form as a system of 'couplings' between the elements 'encountered'. Here again one can assign a probabilistic form to the couplings in order to take account of their complete or incomplete character with respect to a given field of extension. This form is very simple when the couplings are independent (one can interpret in this way the well-known Weber's law for example), but more complex in the case of sequential probabilities, as in successive strategies applied to the solution of one and the same problem (such as the problems of conservation or of construction of the permanent object, examples of which we have given above).

3. An increasing stability of the equilibrium (which does not contradict its mobile character). This will tend to the *minimum* characteristic and to exact compensation (reversibility) of the virtual transformations involved in the system, these transformations being on a higher level than that of the couplings, and consisting in co-ordinating the couplings in various ways.

If we examine by means of such schema, but in full consciousness of our ignorance, present EEG data on the development of the child, we cannot but be struck by certain analogies, which may be superficial or profound. First of all, it is evident that in passing from the slow waves, which are the earliest ones, to the rapid waves, which do not become general until 10 or 11 years of age (at the commencement of the level which we characterize by 'formal operations') we see an advance in mobility; it is, however, correlated with and not contradictory to an advance towards stability and regularity. On the other hand, if we agree with Grey Walter that the alpha rhythm is the manifestation of an exploratory or scanning activity, corresponding to the principle of what we call 'encounters' and giving rise to 'couplings', the progressive extension of the field of this activity from the visual occipital regions towards the temporal and frontal regions is very striking. If one first considers the level of visual perception: we have been able to establish at the Institute, on the basis of the probabilistic schema of encounters and couplings, using over-estimations (and consequently the correlated under-estimations) of lengths, that the lengths perceived are a function of the centration of vision. It is suggestive to compare these facts with the results observed by Grey Walter on the manner in which a perceptual excitation extends further and further into the brain and quickly goes beyond the frontiers of the visual cortex to reach

the motor regions and beyond; this irradiation may thus be related to the opening of pathways by centration. It is very interesting, on the other hand, to note that if, in behaviour, the perceptive couplings are gradually supplemented by representative couplings, bringing about between 12 and 15 years of age a combinatory system proper (commencement of formal operations), we see a correlated extension of the alpha rhythm to regions of the brain which are increasingly more extensive and closer to the paths of association.

In short, even if these attempts to establish two corresponding series of stages for intellectual operations and for the EEGs have so far failed (which may be due, moreover, to the inadequacy of the means of detection used for the latter) nevertheless one may hope to establish more general convergences between the dimensions of the organization of the rhythms and those of the equilibration by successive steps of cognitive links, ranging from perceptual couplings to the combination characterizing formal operations. It would, moreover, be surprising if it were otherwise. For although the EEGs do not of course represent the expression of operations proper, they seem to translate attitudes arising in the cerebral activities of the subject, attitudes ranging from mere watchfulness to active exploration, and becoming differentiated in the field of such exploration into a multiplicity of forms as varied as those seen in observing behaviour. Indeed, if it is legitimate to bring together all types of exploration into a single general schema of encounters, couplings and transformations (or couplings raised to the second power), then one can count on refinements in the analyses of the multiple varieties of alpha waves leading sooner or later to a picture corresponding in broad outline to that of the different types of behaviour.

#### IV. QUESTIONS TO MY COLLEAGUES IN THE STUDY GROUP

The preceding pages (Parts I, II, III) represent an attempt to proound, in a common language, a certain number of common problems, discussion of which could serve as a synthesis for our work on development. Of course, each of us remains free to declare that the problems are ill-chosen and that their discussion will lead to nothing, but showing why they are ill-chosen or will lead to nothing would itself bring about a synthesis.

However, it may be useful in concluding this essay to put a few questions personally to members of the group in the hope of systematic replies. I shall not put such questions to Inhelder or to Zazzo, who are too close to the way of thinking embodied in the essay, and I shall consider Melin, Monnier and Rémond as sharing in the questions put to Grey Walter.

### I. Questions to Lorenz

1. Static apriorism is only an over-simple preformism, which Lorenz rightly rejects and replaces by a 'dynamic apriorism'. However, is there not a risk that the latter will bring us back indirectly to vitalism, i.e. to the convenient theory that 'life' can always arrange everything? Is not the only way of escaping from vitalism, if one is not an empiricist, to have recourse to probabilistic equilibration processes?
2. Lorenz has shown us (Vol. I, p. 197) various compromises between the IRM and learning and above all he has shown us (Vol. II, p. 264) that a characteristic may be innate in the case of one species and based on individual learning in another. Does not this show that the appearance of this characteristic is necessary as part of a certain functional process of equilibration which is more general than the innate or the acquired? This appears to me to be the case, for example, in behaviour entailing search for an object which has disappeared, behaviour which is acquired in the baby (see above III 1 A), but is innate in many animals (digging instinct, etc.).
3. 'Logical necessity does not exist *per se* but corresponds to laws of the nervous system' (Vol. II, p. 264). I agree if these are laws of equilibration such as the law of 'all or nothing' in which has been seen the starting point of binary arithmetic (one or zero), which is isomorphous with Boolean algebra, and consequently with logic. But otherwise we fall back into preformist apriorism.
4. Are there objective criteria for distinction between compromise solutions and more stable equilibrations in cases of conflicting activities (Vol. I, p. 198)?

### II. Bowlby

1. Are there at present any psychoanalytical attempts to explain the *transition* from one stage to the next? Has Bowlby himself considered how to solve this problem? Would he agree with the hypothesis that the reactions pertaining to a stage *n* are set off by dissatisfactions, conflicts or disequilibria pertaining to the preceding stage *n-1*, which hypothesis would favour the interpretation of such transitions on the basis of an equilibration process?
2. What in particular is his attitude to the 'latency' stage? Can one interpret it in terms of equilibrium or is it only the manifestation of a phase of maturation?
3. Does Bowlby agree that all behaviour is *always* simultaneously affective and cognitive, in accordance with two inseparable but distinct aspects, one of which constitutes the *energy component* of this behaviour (affective aspect) and the other the *structure* of the same behaviour (cognitive aspect)? Would he agree in drawing the

conclusion that an affect is never the cause of a cognition, nor the reverse, since both are built up together in an indissociable manner (for example, the cognitive 'permanent object' and the affective 'objectal relation')?

Odier, who dealt with this problem, accepts in all cases the priority and the causal action of the affective on the cognitive which seems to me to complicate matters without yielding any advantage. On the other hand, no one supports the view that the cognitive is the cause of the affective.

### III. *Margaret Mead*

1. Everything varies from one society to another, in particular the systems of numeration and the circumstances under which one learns to count. But why is it generally accepted that  $1+1=2$  or  $2+2=4$ ? This is not innate. It is not learned from experience, since two objects are not equivalent to 'two' unless they are counted (=activity of the subject). Is it 'social' as thought by Durkheim? But he was then obliged to suggest that 'under all civilizations lies the civilization' and consequently to postulate a certain common functioning which seems to me characteristic of the laws of equilibrium (which apply equally well to operations between individuals and to the operations of the individual himself). Does Margaret Mead accept the possibility of arriving, thanks to the mechanisms of equilibration, at such common elements despite the diversity of the cultural points of departure?
2. When an individual is transplanted from one civilization to another or subjected to a new *training*, can any similarity be perceived between the order of things learned during this kind of 'Aktualgenese' and the developmental order observed in the growth of the child as studied among us? Example: the acquisition of the operations of measurement?

### IV. *Grey Walter*

1. Grey distinguished between six possible forms of psychobiological development: (1) evolution (mutation, etc.), (2) tropisms, etc., (3) instincts (IRMs, etc.), (4) learning by repetition, (5) learning by association, (6) social communication (Vol. II, p. 21). I feel that the construction of logical relationships by the child aged 7-8 years (for example, the previously unrecognized relationship:  $A=B$ ,  $B=C$ , therefore  $A=C$ , which presupposes the retention of A, B and C during the process of reasoning) does not enter into any of these six forms, and that we must accept a further category (7) the *learning by successive equilibrations* (individual, or social in the

sense of inter-individual). The examples of cognitive equilibration mentioned in this paper all come within this category (7) and cannot be explained by categories (1)-(6). I should be glad to have the agreement or objections of Grey Walter on this point, since I was not able to make myself understood in London in this connexion (Vol. II, pp. 58-60), when I stressed the fact that, in this kind of learning, knowledge is not drawn from objects but from co-ordinations between the actions of the subject. Consequently, it was my fault that Grey Walter did not reply precisely to my question (Vol. II, p. 60). However, if one is to hope for convergences between the development of the EEG and the evolution of intellectual operations, it is fundamental to know whether such learning by equilibration (of which the finest mechano-physiological model is Ashby's homeostat) reduces to the six forms of Grey Walter or not.

2. The most important critical age for our cognitive stages is, on the average, 7 years. Now Grey Walter has shown (Vol. II, p. 71) that 'elaboration' appeared in the EEG only towards 6-7 years. Could not this 'elaboration', of which there is no trace before 3-4 years, which increases from 4 to 6-7 years and takes on a more general and more stable form from 6-7 years of age, be related to the type of structuration to which we have just drawn attention?

3. Generally speaking, is the inadequate correspondence observed so far between the EEG and the cognitive structures, thought by Grey Walter to be due to gaps in recording, due to the nature of the EEG, or might it not also be due in part to the inadequate theoretical development of possible common mechanisms (I have in mind the development still called for in connexion with a theory of equilibration)?

#### *V. Tanner*

1. In the absence of stages of growth, Tanner recognizes the existence of 'phases of acceleration'. Could these be characterized by the causal mechanism of this acceleration, for example, by a more or less regular interaction between the nervous and endocrine mechanisms, or should one restrict oneself to observing this acceleration as such?

This question is indirectly related to the one which seems to me the most important of the general problems: the mechanism of the transition from one stage to another, the mechanism of continuous transformations.

2. What outstanding transformations in the nervous system might it be possible to relate to the levels of  $1\frac{1}{2}$ -2 years (beginning of language and of symbolic function in general), of 7-8 years (beginning of

complete operations) and 11-12 years (beginning of formal operations, linked with the functioning of the frontal lobes)?

3. Organic embryogeny (which is continued in physical growth) has often been thought of as directed towards the form of equilibrium which constitutes the adult state of the corresponding species. Could one at the present time say something positive regarding the criteria of this equilibrium (*minimum* and compensated virtual transformations), its mechanism (regulations) and above all, the mechanism of the successive equilibration phases (other than the final stage), which might be compared with analogous problems in other sectors of development?

#### VI. *General questions (for all)*

Discussion of this attempted synthesis cannot be fruitful unless each of us, in his own field, supplies a few well-defined and well-analysed facts relevant to the passage from one stage to the following or to some particular continuous transformation occurring in the course of development. It is by comparing such facts that we shall be able to decide whether it is reasonable or whether it is definitely premature to attempt to characterize certain *general* mechanisms of development. I am almost certain that the equilibration mechanisms explain the development of the logico-mathematical operational structures, because these structures are themselves nothing more than the equilibrium forms peculiar to the intellectual operations. But only a general discussion would show whether we have here a possibility for a general or only for a particular explanation.

## Comments on Professor Piaget's Paper

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I think it advisable to answer Professor Piaget's questions to myself first, and then to proceed to what I have to say on the conceptions of 'development' and of 'stages' as well as on the urgent necessity of a 'common language'.

1. Professor Piaget's first question to me was whether there is not a danger of vitalism surreptitiously introduced by my attitude to the 'prioric' forms of thought and categories. He calls this attitude 'dynamic apriorism'—and I think that this term is entirely misleading: I am profoundly thankful that it is so, because any sort of apriorism, however dynamic, would indeed lead to the danger Professor Piaget fears. I am quite convinced that things that conform to Kant's definition of the *a priori*—e.g. things that exist in our mind before any experience and which must be there in order to make experience possible—are not things that exist in the absolute. Nothing is really there *a priori*. All the forms and functions of our mental processes that really exist independently of experience are related to the form and function of our central nervous system and have developed in phylogeny just as have the form and function of any of our other bodily organs. All structures and functions have attained their present form in an age-long interaction between the organism and its environment. Nothing whatsoever is preformed, unless it be the basic properties of the smallest known physical units. Nobody in the world is less of a preformationist than the phylogeneticist. If I may widen the concept of the empiric so far that it includes not only what the individual derives from personal experience, but everything that the species gains out of its interaction with outward reality, then I should definitely call the attitude assumed by ethologists towards the problem of the '*a priori*' one of an extreme 'phyletic empiricism'.

2. The second question, if I understand Professor Piaget rightly, is whether a process of functional 'equilibration' is not much more general and primary than the function of innate releasing mechanisms and learned responses (see page 24). If I may substitute 'adaptive interaction' for 'equilibration', as I assume I may, the answer is

simply and emphatically yes. (I agree with Professor Bertalanffy's objection to the term equilibrium: see on page 94 below). There definitely are organisms which do not have any instinctive movements or innate releasing mechanisms and also are quite incapable of learning. All organisms are open systems and all of them live only by achieving a regulative equilibration between their inner processes and the requirements of their outer environment. The functions of innate releasing mechanisms and of learning are those only of very highly specialized organs that higher animals have developed under the pressure of natural selection in the service of that general regulative equilibration. The same applies to searching behaviour, to all cognitive functions, in short to all structures and functions which develop a survival value. I do not think that the term 'compromise' (p. 24) is very descriptive for the co-operation of the innate and the acquired. An organism can be 'constructed' in very different ways by all the factors affecting evolution, of which I still think natural selection to be the most effective. A grebe is 'so made' that it needs to learn very little in order to survive, having beautifully specialized innate responses and organs. But a raven needs a lot of learning and correspondingly is furnished with an inexhaustible source of exploratory behaviour: both 'constructions' are equally successful in surviving.

3. The third question concerns my statement that 'logical necessity does not exist *per se* but corresponds to laws of the nervous system'. Professor Piaget fears that the acceptance of existing 'laws' may lead back to preformist apriorism. It does not, though, because the 'laws' in question are by no means logical necessities. None of the biological 'laws' are. Mendel's 'laws' would be entirely different if the structure of chromosomes and the processes of fertilization were not exactly as they are, which might easily have happened if evolution had run a slightly different course. Exactly the same applies to all the 'laws' prevailing in the function of our brain.

4. The last question is whether there are any objective criteria for distinguishing, in cases of conflicting motivation, mere compromise solutions from more stable equilibrations. It is one that is occupying ethologists most seriously. Indeed, the distinction between a mere epiphenomenon and a function which serves 'equilibration', in other words one that develops a definite survival value, is, in many cases, of the utmost importance. It can, however, only be answered for each single case separately and only by a thorough experimental investigation.

I now come to the question of common language which is more or less identical with the problem of synthesis. I confess that I heard of general system theory for the first time when I read Professor Bertalanffy's comments (see page 69), so I know no more about it

than what he said in his first three pages. My question to Professor Bertalanffy may therefore be quite beside the point: but is there not a certain danger that, in order to make different systems comparable and describable in the same 'language', we strip them of characters which seem to be non-essential frills from the point of view of theory, but which are highly characteristic and essential to the proper understanding of each of the systems separately?

On the other hand, the study and comparison of extremely different systems may reveal the surprising fact that they contain mechanisms that *are* directly comparable. Modern physiology of perception in particular and neurophysiology in general have discovered processes which are not only comparable, but essentially identical with those known to cybernetics. I entirely agree with what Bertalanffy says about the danger of using fashionable words in a loose way, but this is certainly not the case when Mittelstaedt or Von Holst use cybernetic terms in their studies of optokinetic movements or the function of the muscle spindles. Indeed, the processes investigated in these cases are classically simple examples of positive and negative feedback mechanisms, and it would be a great error and hindrance to mutual understanding not to use such terms.

Another example: at our last meeting I was trying to explain the controlled use of Gestalt perception in the study of animal behaviour (Vol. III, p. 122). I am afraid it took me a very long time to expound how very many repeated observations of the same process are necessary before our Gestalt perception at last succeeds in disentangling the essential lawfulness from the 'background' of inessential, accidental sensory data. Grey Walter was sitting beside me and, looking over his shoulder, I was slightly taken aback to see that he had compressed the whole symphony of what I had been trying to explain into one sentence. He had written: 'Redundancy of information makes up for noisiness of channel'.

This is an example of a *perfect* translation of the kind that general system theory should strive for. But we must keep in mind that this kind of mutual understanding is only possible wherever two independent investigations have reached a comparatively high degree of insight into the process investigated. Gestalt perception is a function dependent on a neural organization that is very much akin to a true computer and which consequently lends itself particularly well to a description in the terms of information theory.

In the majority of cases, however, our insight into what really happens in an organism is much too superficial to permit a translation that is similarly fundamental. We must never forget that the words we use are connected with conceptions of vastly different degrees of clarity. If I speak in the same breath of instinctive movements and of innate releasing mechanisms, I cannot help suggesting,

in a most insidious manner, that the conceptions symbolized by these two words are of approximately equal value. They are not. We can make, to say the very least, a pretty shrewd guess as to the physiological nature of instinctive movements, while we have but the haziest ideas concerning the physiological mechanisms underlying the function of an innate releasing mechanism. Therefore, what ethology calls instinctive movements can be described tolerably well in the terminology of Von Holst's studies on central co-ordination, while the conception of the innate releasing mechanism which is only functionally determined cannot be translated into anything at all until we know much more about it than we do at present.

Nevertheless, these hazily defined conceptions correspond to something real. I have much confidence in the ability of our Gestalt perception to pick natural units out of the immeasurable chaos of sensory data. If an observer like Piaget calls something 'affectivity', I rely blindly on the assumption that there is a natural unit corresponding to that term. But I find it very difficult to ascertain what exactly that unit is. All conceptions of this type are what Hassenstein has called 'injunctive'. *Injungere* means to enjoin. A number of characters are 'enjoined' in order to make a special case fit into the contents of the conception. A number of constituent properties go into the making of the conception, but none of them ever is 'constitutive': they constitute the conception only by a process of summation. A special case may lack one or even several of these properties, and yet not be excluded from the contents of the injunctive conception. Metabolism and reproduction are indubitably constituent characters of life, yet a cooled anthrax spore which has no metabolism, or an ox which cannot reproduce, are unquestionably alive. Symbolic speech is a constituent character of Man, yet a patient with total aphasia still is human, etc., etc. All injunctive conceptions merge, without any clear boundary-line, into neighbouring ones which have one or several part-constituent characters in common.

All the words which we coin to describe natural units, of whose existence we are told by our Gestalt perception, necessarily refer to injunctive conceptions exclusively. When we first say 'bow-wow', we do not ourselves know whether we mean this dog, any dog, any mammal, any four-legged animal or perhaps anything alive. It is quite difficult to find out what part-constituent properties one enjoins oneself, if one wants to place a special case under the heading of an injunctive conception. And it is still harder to know exactly what another man is enjoining when he uses the same term. Injunctive conceptions may not only vary as to the size of their contents, but their contents may overlap. The trouble is that real natural units may overlap. Take a zoological example. Every naïve person seeing a lamprey for the first time would say it is a fish. It has eyes, gills, a

silvery surface, etc., just like any other fish: but it has no jaws. Anybody with an inkling of comparative anatomy would see in an instant that a shark, a frog and a man are more closely related to each other than all of them are to a lamprey. 'Fish', including the cyclostomes, are a natural unit, and 'fish', as a class of gnathostomes, excluding the lampreys, are also a natural unit. Which sort of unit is reported to a given man by his Gestalt perception, and what he consequently subsumes under an injunctive conception, depends on the man.

Consequently, you have to know that man and his whole way of thinking and observing just in order to know what he means when he uses one single word. And the more of an observational genius the man is, in other words, the more unexpected natural units his Gestalt perception makes visible to him, the more difficult we shall find it to get hold of the part-constituent characters that make up his injunctive conceptions. Indeed he will find it so himself! I am sure that Professor Piaget will take it as the compliment which is meant when I say that he is a *very* difficult man to understand—in the respect just discussed! I do *not* know what he means, for example, by the word 'affectivity'. John Bowlby, in his comments, has attempted to translate it into ethologese, defining the conception exactly as I would, but I do not expect Professor Piaget to feel himself very deeply understood.

On the whole I think that we have done marvellously well in learning to understand each other. A good symptom of this is if one finds oneself adopting another person's concepts—not the word, mind, but the concept. Speaking for myself, I have done that extensively. The conception of the case-history, which formerly did not play any role at all in our daily work, now looms very large indeed. Conversely, I find some of our study group, particularly Bowlby, using ethological terms naturally and correctly.

Correct mutual understanding, in other words, exact coincidence of conceptual contents correlated to the words used, is, of course, the primary condition without whose fulfilment there is no hope for a real synthesis of several people's work.

Synthesis of several people's work is nowhere more necessary than in the study of development. This term is, of course, again correlated to an injunctive conception of immense complication. But in the case of words used in common parlance it is, on principle, not necessary to go into a detailed conceptual analysis in order to achieve mutual understanding. We are, I think, all agreed upon what development is and I may start what I have to say about the synthesis of our work by quoting Goethe's old definition: 'development is differentiation and subordination of parts'. The two hemispheres of a globular, blastula- or volvox-like creature divide the functions of

nutrition and defence between themselves, each of them specializing for one of these tasks and consequently becoming as different from the other as ectoderm and endoderm are. By the same act, they become more 'subordinated' to the whole system, as they become dependent on each other, each being incapable of fending for itself. This clearest and most primitive division of labour that ever took place in a metazoan ought to furnish a good example of what 'development' is like and how it ought to be approached in theory. The change of each part has a counterpart in the change of all the others. 'Differentiation' always means 'becoming different' and the question 'different in relation to what?' ought always to be in our minds. In the case of the literal and spatial differentiation of the blastula this question is easy to answer, and it is still answerable in the early stages of embryonic development in which a comparatively small number of tissues have become different from each other so that it is still possible to keep track of the interactions of their functions. Physiologists of development have done amazingly well at these particular tasks. We, of this study group, ought to take the work of experimental embryology as a model, if only to make ourselves realize how immensely difficult our problems are. Bowlby has already proposed a view of psycho-physiological development which makes use not only of Goldschmidt's principle of harmonized reaction velocities (page 36 in his comments see on); he has also, without explicitly saying so, introduced another indispensable concept of experimental embryology, that of 'regulative' and 'mosaic' interaction between the developing parts. Luckily for the analytic biologists, organisms are not 'wholes' in the sense that 'everything' is in a regulative interaction with everything else: there are some few relatively autonomous structures which influence the rest of the system far more than they are influenced by it in return. These are the Archimedean points on which to base investigation. These comparatively invariable and autonomous elements are necessarily more often causes than they are effects in the immensely complicated network of interactions taking place in development. For the same reasons for which investigation and didactic representation of the whole organism invariably start from its skeleton, we ought to try first to get hold of the most autonomous and independent processes of structural and functional development.

Another reason for doing this is that the harmonization of reaction velocities is most liable to go wrong or to fail in regard to these relatively autonomous processes. I think that Kretschmer is entirely right in attributing a large number of psychological disturbances to the disharmonization of the velocities with which a number of structures and/or functions develop in an individual. In the greylag goose, that invaluably simplified 'model', we found that practically all

disturbances of sexual function are due to disharmonization of developmental velocities in relatively autonomous activities. Oedipus behaviour arises in exactly the way Kretschmer supposes and male homosexual pairs are formed when a certain stage of courtship activities is 'skipped' because of environmental conditions which prevail in a state of semi-captivity but which may also, often enough, occur in the wild. Helga Fischer has recently found a highly interesting mechanism by which these homosexual pairs are broken up later on and the partners brought back to 'normal'.

Even in geese we find it quite unfeasible to describe 'stages' in the development of behaviour *as a whole*. Well defined 'stages', however, are found in the development of single, relatively autonomous activities and well defined types of disturbances can be correlated to the temporal lack of coincidence of stages, particularly in individuals with a certain amount of domestic inheritance. But also in pure-blooded wild birds the variation of developmental velocities in different activities is so enormous that it would need a very forced and artificial abstraction of a type termed 'normal' to make it possible to speak of 'stages' in the development of the whole organism. I confess that I have very strong doubts whether the variability of developmental velocities in the child is any less than it is in the wild goose and I therefore emphatically agree with the objections to the typification of 'stages' in the development in humans. I have no doubt that very real 'types' of personalities can be explained on the basis of coincidence and non-coincidence of stages in the development of relatively independent structures and/or functions.

The 'moral' of all this is perhaps a platitude: each of us ought to be constantly conscious of the fact that he is only investigating the development of a very small part-structure and/or function. Each of us ought to be looking constantly for lawful coincidences and non-coincidences between the 'stages' in the developmental processes he investigates and those that some one else is studying. Each of us ought to be searching constantly for lawful and harmonizing interactions between the processes he himself is working on and the most unexpected and far-fetched developments in other parts of the organism, even if the latter do not interest him in the least. But we ought not to postulate *a priori* that any particular interaction exists. We know there are highly independent mosaic parts and whether or not they interact, and if they do, to what extent, are problems that must be investigated singly for every single case.

## Comments on Professor Piaget's Paper

*Introduction*

Piaget has raised some very fundamental problems and in attempting to answer the questions he has put to me I have found it necessary to give some indication of my own position with regard to them. This I do with some diffidence, partly because of the difficulty of the issues raised and partly because I am often unsure how adequately I have comprehended Piaget's ideas. In preparing the following comments I have been much helped by three research colleagues: Peter Hildebrand, whose knowledge of Piaget's writings is far greater than my own, Anthony Ambrose and Robert Hinde.

Though I find myself in close agreement with the early passages of Section 1 of Piaget's paper, I find difficulty over his concept of affectivity and I expect to find much more autonomy in the development of different structures and their related activities<sup>1</sup> than Piaget seems to expect. However, before commenting on this I think it would be best for me to deal with the problems of stages of development since this is where ideas derived from psychoanalysis are much concerned.

*Problem of stages of development*

In discussing this topic I want to bring out rather fully a distinction which is clearly present in Piaget's discussion through not always very explicit. I do so because I think there is always a danger of confusion arising if we do not clearly distinguish:

- (a) phases in the development of the whole organism,
- (b) phases in the development of particular structures and activities of the organism.

Some of the most dramatic examples of phases in the development of the whole organism are apparent in insect life, e.g. larva, chrysalis,

<sup>1</sup> To describe the growth of any particular part of the organism, I am referring to the part as a 'structure and its related activity' to make it plain that structure and activity are indissolubly linked. To avoid clumsiness in the text I have often contracted it to 'structure and activity' or even to 'activity'.

imago. In mammals one can easily discern the intra-uterine and extra-uterine phases, but when one tries to find substages within the extra-uterine this type of classification quickly breaks down. The second concept—phases in the development of particular structures and activities—seems to me far more valuable than the first.

In the field of physiological structure and activity, development, though in some respects gradual, often proceeds in steps, e.g. when the foetal heart begins to beat or when the child starts to walk. Tanner has emphasized, if I understand him aright, that in the physiological field different structures and activities often develop at different rates and that a major step in development in one area may not be contemporaneous or even in any obvious way co-ordinated with one in another area. Perhaps a partial exception to this in mammals is birth, where several activities change in character simultaneously, though even here I imagine those which change constitute only a small minority of all the activities operating. Thus in physical growth the picture appears to be one of a multiplicity of developing activities each progressing at its own pace and passing through major phases which may not be closely co-ordinated with the phases passed through by other activities. As a result it is not possible when we consider the organism as a whole to discern overall stages of physical growth.

It is my impression that exactly the same picture will be found in the development of psychological structures and activities. Piaget clearly postulates multiple psychological activities; I am not clear how many he expects to find but I have the impression it may be only a few. My own expectation is that we shall identify very many. Furthermore, whereas Piaget seems to expect to find fairly close parallelism and interconnexion in their developments, I do not. In so far as development in any one area is influenced by the total field of forces in which it is occurring there must be interconnexion, but I expect it to be complex in its manifestations with each structure and activity developing at its own pace.

It seems to me that the view that psychological development is the product of the relatively autonomous development of a fairly large number of different structures and activities agrees with clinical experience of the ordinary child: the development of each child is extraordinarily uneven in respect of different activities and every child differs from every other in the order in which they develop. This is a view of psychological development which makes use of Goldschmidt's principle, derived from embryology, of harmonized reaction velocities; this principle he advanced to account for physiological development and the differences in its outcome displayed in the mature form reached by each individual organism. Just as, according to Goldschmidt, differential variations in the velocity of

development of different structures and activities account for differences in the mature physical form of organisms even as far as the differences between the sexes of one species, so would I expect variations in the velocity of development of different psychological activities to account for differences in the psychic form or personality of individuals, including differences which tend to be characteristic of the two sexes. In other words I am pinning much hope to the systematic application of Goldschmidt's theory to psychological development.

Let us now turn to the various theories of stages of development which have been advanced in regard to children. Wallon's stages seem to be concerned with the organism as a whole and are therefore in my view of limited use. The stages described by Inhelder and Piaget are concerned exclusively with cognitive structure and activity, where they appear to be of the greatest value. As indicated earlier, however, I do not share Piaget's hope that they will be found to run closely parallel to the development of other activities. Finally, there is Freud's formulation of the theory of libidinal phases.

#### *Stages in the organization of the libido*

First, it seems plain that in advancing his hypothesis Freud is referring to the development of a particular function, namely the sexual. Any attempt to extend these stages to characterize the whole of the psychological development of the child would seem to be wholly mistaken.

However, with Piaget, I think Freud's libidinal phases differ materially in their nature from the cognitive phases of Inhelder and Piaget. The latter are characterized by true steps, namely the condition of progress from one phase to another is that the prior phase is a necessary precondition for the emergence of the subsequent one. At the most Freud's libidinal phases are, as Piaget points out, only phases with dominant characters. Freud (1949a) himself remarks: 'It would be a mistake to suppose that these three phases succeed one another in a clear-cut fashion: one of them may appear in addition to another, they may overlap one another, they may be present simultaneously'. Such an assessment seems to me to rob the notion of libidinal stages of most of its usefulness. It happens that in contrast to many psychoanalysts, I have never found the concept of libidinal stages useful and I regard the elaboration of hypotheses which seek to relate particular psychiatric syndromes to particular libidinal phases as mistaken. For all these reasons I recommend we do not spend too much time on them.

It happens, however, that mixed up with Freud's concept of libidinal phases there is what in my opinion is a far more valuable

idea; this is his notion of 'individual component instincts' which are at first 'disconnected and independent of one another' but which later 'under the primacy of a single erotogenic zone, form a firm organization directed towards a sexual aim attached to some extraneous sexual object' (Freud, 1949b). The theme here is that the sexual responses of the mature adult are to be seen as the result of a special synthesis of a number of component behaviour patterns, some of which first make their appearance in infancy and early childhood. Freud's further point is, of course, that sexual disturbances including perversions are to be understood as resulting from a faulty synthesis of these components. These ideas of Freud seem to me to be almost identical in character with ideas now fairly widely accepted by ethologists in accounting for the complex behaviour of other species, e.g. nest building or courtship in birds.

If we select this aspect of Freud's formulations, we see that the phases of sexual development which would correspond to Piaget's phases of cognitive development would be not the manifestations of component patterns, but the various steps in the synthesis of these behaviour patterns into a more complex whole. I am not aware that any systematic work has been done on this though I may well be wrong. If the orientation I am recommending is a useful one, the tasks before the research psychoanalyst are (a) to describe more carefully the component 'part-instincts', (b) to study the stages in their synthesis to form mature sexual behaviour in its varying forms, normal and 'abnormal'.

Here I would like to say a word about the terms fixation and regression. Both are used in at least two different senses.

*Fixation* can refer either to a pattern of behaviour or to the object towards which the behaviour is directed. In the second usage it seems to me fairly satisfactory, though it tends to be used to refer only to 'abnormal' object choice, whereas a neutral term referring to the selection of any object, normal or 'abnormal' would be better. The term when used to refer to the persistence into adult life of behaviour patterns characteristic of infancy or childhood can be misleading and smacks too much of a static tethering to the past. Instead, as Piaget and I have agreed, it is far more fruitful to think of present behaviour as being due to the ongoing dynamic assimilation and restructuring of the past in terms of the present.

The term *regression* is often used by psychoanalysts in rather the same sense as the first usage of fixation: but, whereas fixation usually refers to the *persistence* into mature life of patterns of behaviour characteristic of the immature, regression is often used to describe a *recurrence* of such behaviour after it has been discontinued. This usage should of course be distinguished sharply from the usage such as that which Lewin (in Barker *et al.*, 1943) adopts in discussing

his experiment on Frustration and Regression. Here it is used to describe a return to less differentiated behaviour by children capable of more differentiated behaviour. It is imperative we find two terms to describe these two different processes. My own inclination would be to coin a new word for the concept denoted by the psychoanalytic usage; in coining it I would strive to convey the meaning of 're-arousal'. Unfortunately, however, it would hardly be easy to persuade psychoanalysts to forego their traditional use of the term.

Piaget (p. 19) is still not quite convinced that I share his belief that present behaviour is a result of the ongoing dynamic assimilation and restructuring of the past in terms of the present. Actually this is a view which many English psychoanalysts have emphasized for ten years or more as a result of being influenced by Lewin's field theory. The late John Rickman (1951) (President of the British Psycho-Analytical Society, 1947-50) was an ardent exponent of it and most of the analysts associated with the Tavistock Clinic and Institute share this view quite explicitly.

Having lived in this intellectual climate for some years I at first found the delighted incredulity with which Piaget has greeted my agreement with him a little puzzling. However, I have recently come across a passage in one of Freud's later papers which makes it plain that Freud never reached this view himself but, on the contrary, was a convinced exponent of the opposite standpoint, which I am afraid a majority of analysts still adopt. In contrasting the work of the psychoanalyst with that of the archaeologist who has to make reconstructions from material much of which has been lost or destroyed, Freud (1950) writes 'But it is different with the psychical object whose early history the analyst is seeking to recover. . . . All of the essentials are preserved, even things that seem completely forgotten are present somehow and somewhere, and have merely been buried and made inaccessible to the subject. Indeed, it may, as we know, be doubted whether any psychical structure can really be the victim of total destruction. It depends only upon analytical technique whether we shall succeed in bringing what is concealed completely to light.'

From this and other passages in the same paper it must be admitted that Freud held the view (*a*) that analytic treatment was concerned with 'digging up the past'—'we are in search of . . . a picture of the patient's forgotten years that shall be alike trustworthy and in all essential respects complete', and (*b*) that he believed that accurate reconstructions of the past could be achieved in the course of analytical therapy. As I explained in my contribution to our third meeting (Vol. III, p. 159), I and many English analysts do not share this view of therapy. Moreover, my own research programme is based on the view that data obtained in the course of analytic

therapy can only be samples of behaviour (including introspections) which, though much influenced by the past, are inevitably influenced also by the present. For this reason what these data tell us of the influences active in the patient's early years is seen as in a glass darkly. Therefore if we wish to know about the influence of early experiences we have no option but to study the individual undergoing them as and when they are occurring.

Incidentally it is useful to note that we can formulate in two ways the process Piaget and I believe occurs. On the one hand we can refer to present behaviour as due to ongoing dynamic assimilation and restructuring of the past in terms of the present: on the other we can say that the present to which the behaviour is a response is assimilated and structured (or interpreted) in terms of the past.

### *Affectivity*

I find it rather difficult to be clear what Piaget has in mind by this word, which happens not to be one I use. I get the impression that he thinks of something unitary in character in the same way as he thinks of cognition as unitary. My own outlook is probably radically different. I think of affectivity as the accompaniment of an activated behaviour pattern, each behaviour pattern having its own characteristic affectivity. In the following account I realize I am following closely the ideas put forward six years ago by Lorenz (1950).

### *Behaviour patterns*

In putting forward the following ideas I realize I am giving a rough and ready sketch map involving nothing less than a theory of motivation and affect. My ideas are anything but clear and I am only advancing them now in order to give Piaget and others an impression of the lines along which I am thinking. The extent to which I have been influenced by ethological data and theory (much of it culled during discussions with Robert Hinde) will be evident.

Affect laden behaviour I tend to view in terms of structures built of component bricks. The bricks are relatively stereotyped behaviour patterns, e.g. bird song or sucking, which, according to the species, may be built in or learnt or a combination of both. The larger structure, e.g. courtship or nest building, is less stereotyped and a complex synthesis of these components. Although in principle any component is available for any synthesis, in practice each synthesis tends to select a particular group of components. None the less it is probably usual for certain component items to be utilized in more than one synthesis.

A typical example from the bird world is seen in connexion with

courtship feeding where, in many species, as part of the courtship the male feeds the female in a way clearly resembling the feeding of the young by parents. There are two patterns to concern us here: (i) the food presentation of the male, (ii) the food begging of the female. Food presentation can be seen as a component pattern of the two more complex behaviour sequences (a) parental behaviour and (b) male courtship behaviour; similarly food begging can be seen as a component pattern of (a) chick behaviour and (b) female courtship behaviour.

It is my suspicion that in humans the same principles hold, though, because of our much greater capacities both for conceptualizing and for learning, their manifestation is far more complex. I suspect we shall find them especially clearly when we analyse the component patterns concerned in the three basic social relationships—the infant-parent, the parent-infant, and the sexual. For instance, I think we can identify a number of component behaviour patterns, largely if not wholly built-in, concerned in these three relationships; examples are smiling, crying, cuddling, sucking, the pelvic thrust. Some of these component patterns, e.g. smiling, are usually utilized in all three social relationships and some are confined to two or perhaps only one.

It will be seen that this theory has much in common with Freud's. The main difference appears to be that whereas Freud appears to think that the whole of the earlier behaviour patterns are organized into later sexual behaviour, I am suggesting that it is only some components of those earlier patterns which are so organized. Though this is not a negligible difference the approach is manifestly the same.

If the view I am advancing is right, one of our first tasks must be to identify these component behaviour patterns and, later, to discover by what means they are selected and synthesized to become parts of greater wholes. As regards the latter we would have to keep in mind the concept of harmonized reaction velocities and the big differences in outcome which would result from even small differential changes in these velocities, whether they were principally the result of the influence of genes or of the environment.

It will have been seen that I do not look on the affective aspects of behaviour as being readily described in terms of gain and loss. I think of behaviour patterns with their corresponding affects as being activated and terminated; from this point of view I am doubtful whether concepts of gain and loss are relevant.

#### *Primacy of social responses*

It should be noted that in following an ethological approach in the foregoing exposition I have been making an assumption regarding the

primacy of social responses about which I ought now to say a word.

Piaget quotes and seems to agree with Freud in his supposition that the only reason that the baby makes social relations is because he learns to do so: by relating to his mother he discovers that his physiological needs for food and warmth are met. This can be called 'the cupboard love theory of infant love', or as Piaget describes it, a 'remunerative strategy'.

Although many psychoanalysts, including Anna Freud, continue to adopt Freud's views on this topic, many others do not. In particular it is called in question by Melanie Klein and many other English analysts, including myself. The issue is basic to all work on social development and many debates between clinicians or between research workers can be traced to contrary views about it. Not only are psychoanalysts at variance among themselves here but the same is true of experimentalists. While those who follow the ethological tradition tend to assume that social responses are as primary as physiological responses, those who follow the learning theory tradition tend to assume they are secondary and learnt. Wallon, I gather from Zazzo, assumes them to be primary.

It is my hope that in the work we are doing at the Tavistock on smiling and crying we may gradually accumulate data which will assist in the resolution of this conflict.

*Relation of affective to cognitive* (see Piaget's third question to me, p. 24)

I find myself puzzled by Piaget's tendency to see the affective aspect of behaviour as dynamic and the cognitive aspect as structural, and I doubt whether this will prove a useful way to look at things. Nor am I inclined to think that 'all behaviour is *always* simultaneously affective and cognitive according to two inseparable but distinct aspects', which is the third of Piaget's questions to me. This is clearly a thorny problem but one which I suspect to be of the utmost importance.

It seems to me that any given pattern of behaviour can, at different times, vary in the amount of emotional and intellective activity which goes with it. For instance at one extreme we know from the work of embryologists (Coghill, Paul Weiss and others) that, at any rate in amphibia, basic patterns of motor co-ordination can develop without major impairment even when the sensory nervous system has been anaesthetized. 'The one fact that has been conclusively established by experimental results is that the central nervous system develops a finite repertory of behavioural performances which are prefunctional in origin and ready to be exhibited as soon as a proper effector

apparatus becomes available' (Weiss, 1955). This suggests to me that it is useful to look at the development of behaviour patterns, even affectively toned ones, as being possibly independent of cognitive development in their initial stages.

At the other extreme are cognitive activities, e.g. solving a mathematical problem, which seems to be almost or quite independent of any behaviour pattern.

Furthermore, there are many patterns of behaviour, both learnt and unlearnt, which can vary in their cognitive component from occasion to occasion. An example of the unlearnt is breathing, which usually has negligible emotional or cognitive components but may acquire both if suffocation is imminent. Examples of the learnt are serving at tennis or playing a well-known piece on the piano; both skills have been acquired through cognitively directed effort but both may later be performed almost automatically, the established behaviour pattern taking charge.

I realize that in this context I am using the term 'behaviour pattern' in rather a broad way to include a range from co-ordinated movements like walking or breathing which are to a high degree built in to the CNS during its maturation and are dependent on particular muscle groups, to extremely complex movements which are wholly learnt and which are far less dependent on individual muscles. (A good example of the latter is one's signature which, although usually effected with one's hand, is similar in form when produced by any group of muscles, e.g. those of one's leg and foot when signing one's name in the sand with one's big toe.) It is my belief that this extended use of the term 'behaviour pattern' will prove justified because I suspect that both the unlearnt and the learnt may prove to have basic characteristics in common.

It is clearly one of man's special characteristics that he is able to acquire through learning such an extraordinary diversity of new patterns. Further, as Paul Weiss (1950) has shown, it is one of man's special characteristics that he can suppress an in-built pattern and utilize instead a learnt one. Whereas a polio patient can learn to use a limb efficiently after tendons have been transplanted and the inborn pattern thereby made inappropriate, rats cannot do so at all and monkeys show only a faint trace of such adaptive adjustments: animals of both species are restricted to the original motor patterns despite the tendon transplantation having caused its activation to result in incongruous and maladaptive behaviour.

Clearly the development of all the more complex learnt behaviour patterns is dependent on cognitive activity. Furthermore, although I have not grasped fully the implications of Piaget's notion of reversibility as the special characteristic of intelligence, I imagine that reversibility is always a characteristic of the more complex

behaviour patterns which man is capable of learning. I shall look forward to Piaget's views on this.

This leads me to what I suspect is a crucial feature of neurotic behaviour. We know that neurotic behaviour is unconsciously motivated and tends to be maladaptive and repetitive: it is felt as irrational and alien to the personality. Evidently such behaviour is still governed by rather primitive processes. Having, as we know, its roots in infancy and early childhood, some neurotic behaviour may well be due to the activation of almost unmodified in-built behaviour patterns, whilst most of it seems likely to be due to the activation of the kind of behaviour patterns which are partly in-built and partly learnt in the earliest years, and whose cognitive components are therefore still primitive. Piaget emphasizes that during early childhood 'we find only unidirectional or irreversible actions' and that reversibility only develops later. All we know of neurotic patterns therefore suggests that their cognitive component lacks that 'most specific characteristic of intelligence', namely reversibility.

I am wondering what Piaget will think of this suggestion and whether he accepts the corollary that, at all ages, behaviour is regulated by cognitive processes of different degrees of development—that in some of our actions we operate with a fully-fledged intelligence characterized by reversibility and in some with an extremely primitive intelligence or none at all, and that in respect of any one activity we may shift from one level to another?

In this connexion Paul Weiss in his studies of polio patients has demonstrated that, even after a new pattern of functioning has been learnt, 'patients would frequently relapse into the old incongruous pattern', demonstrating that 'the latter remained latent but retained its integrity and reappeared periodically whenever the higher replacement went into recess'. This line of thought is fairly consistent with Freud's conception of psychoanalytic therapy. Making what is unconscious conscious is usually interpreted to mean removing barriers between different dynamic systems; such a process may perhaps also involve raising the cognitive component of a behaviour pattern from a primitive to a more sophisticated level.

The upshot of all this appears to be that, in contrast to Piaget, I am inclined to think of some behaviour as being structured in its own right and independently of any cognitive aspect it may later acquire, though I realize that the more complex behaviour patterns are probably structured cognitively from the start. Whether conversely cognition can ever be regarded as dynamic in its own right I find hard to know. Similarly, I have difficulty with regard to the second part of Piaget's third question to me, namely whether affect is ever the cause of cognition or the reverse. My inclination is to suppose that cognition only develops as a result of behaviour and

is therefore secondary to it, but I would like to consider the evidence more carefully before giving any definite opinion. From what I have already said, however, it will be seen that, at least in respect of primitive and more or less built-in behaviour patterns, I do not expect to find any very close parallelism in development between the cognitive and the affective.

Turning to the various hypotheses regarding the degree of synthesis which we may hope to attain, which Piaget lists at the end of his Section II on The Problem of the Stages of Development (p. 13), it will be seen that I favour hypothesis 1—that there are no general stages and that 'we see an intermingling of processes of development which are evidently interrelated, but to different extents or according to multiple temporal rhythms, there being no reason why these processes should constitute a unique structural whole at each level'. I do not favour hypothesis 3, that the personality is built up by successive stages of equilibrium and that it will be possible to establish correspondences between stages in the different studies we undertake.

Before discussing the question of transition between stages, I want to say a word about equilibrium.

### *Equilibrium*

It is plain that the structure and activity of the organism as a whole cannot be understood simply in terms of structure and activities of its parts and that the process of organization of the separate activities into a whole must have laws of its own and that, in so far as the organism persists and develops, there must be an equilibrium of forces. In considering the propositions advanced by Piaget, with which at present I am very unfamiliar, I should be concerned to ensure that they take account of various nonadaptive outcomes which favour the survival neither of the individual nor of the species. Examples from my own field would be suicide, a mother murdering her baby, and the affectionless character following prolonged separation. As regards the latter we know it to have tremendous stability but it is certainly inimical to the individual's capacity for participating in family and social life.

I imagine in considering equilibria we have to distinguish rather sharply between the particular outcomes, some of which may be nonadaptive, and the system of feedbacks and governors which, because they more often than not lead to an adaptive outcome, are biologically reasonably efficient.

*Transition from one stage of development to the next* (see Piaget's first and second question to me, p. 24)

This is clearly a crucial issue and there is a good deal of psychoanalytic literature on the subject. For instance there has been much

discussion of the factors which lead to a fixation at a particular libidinal phase; the traditional hypotheses are:

- (a) that a particular part-instinct is innately overstrong and
- (b) that environmental factors account for part of it.

Of the latter, both frustration and overgratification have been incriminated. (I believe the evidence that frustration is relevant is strong; I am much less convinced about the evidence for overgratification.)

Moreover Melanie Klein, who has done much to call attention to the crucial importance of ambivalence to the love object, has elaborated hypotheses regarding modes of resolving the conflict of ambivalence and transition from one mode to another (see Klein, 1952). Although I am in close sympathy with her general approach I do not find the details of her formulation very convincing. In particular I think it more likely that some of the processes she described as occurring in the first few months in relation to the breast occur during the second year in relation to the mother as a whole person. Her views are almost entirely based on a reconstruction of what may happen in the first year of life using for the purpose data obtained from the analysis of patients above the age of two years. In my view little progress will be made in the theoretical debates which have arisen around Melanie Klein's hypotheses until systematic observation, and where permissible experiments, are made on infants in their first year.

In respect of first-hand experience the transitions with which psychoanalysts are most familiar are those to be observed during successful therapy. Although occurring much later in the life history than is optimal, such transitions are widely believed by psychoanalysts to resemble, in some measure at least, those which are usually achieved in the early years. A systematic study of the nature of the therapeutic process should cast light on certain important transitions, for instance those concerned with developing more efficient modes of dealing with the conflict of ambivalence. To return to Piaget's questions. I would like further information about the implications of his 'hypothesis that the reactions to a stage  $n$  are released by the dissatisfactions, conflicts, or disequilibria belonging to the previous stage  $n-1$ '. Many transitions in behaviour and psychological function I suppose to be due to the maturation of the CNS and the bringing into use of new groups of cells and new circuits. A case in point would be the transition in the development of motility from crawling to walking. Development of cognition must also be dependent on maturation of the CNS, though experience and learning obviously play a larger part than in the development of walking. I conceive of development in the behaviour patterns concerned with social interaction as being dependent partly on CNS

maturation and partly on experience, and in this respect comparable with the development of cognition.

In so far as all growth and development can presumably be traced to biochemical disequilibria I agree with Piaget's formulation. I am doubtful if I agree with it if the 'dissatisfaction, conflict or disequilibria' are conceived of as purely psychological.

As regards the *stage of latency* (Piaget's second question to me) I am inclined to regard it as the manifestation of a phase of maturation which in considerable measure has been built into the human species in the course of evolution. Nevertheless I believe the form it takes to be highly dependent on experience.

As regards Piaget's final question addressed to all of us, there are two areas of transition which I might reasonably attempt to study. One would be the integration into an organized whole of the behaviour patterns which form components of the infant's relation to his mother. I am thinking here of suckling, smiling, crying, the need for physical contact, etc. Before attempting to do this, however, I think it will be necessary to do a good deal more research on the nature of these different responses. The other field would be in the transition from one mode of handling the conflict of ambivalence to another.

## Comments on Professor Piaget's Paper

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In replying to Professor Piaget's challenging paper, I find that I must first distinguish between two approaches which appear in his statement. On the one hand he appears to say that in order to have a unified theory of development, itself dependent upon a common language, which will make it possible to bring our various materials together, we must recognize the three traditional divisions of (a) hereditary factors, (b) the action of the physical environment, and (c) the action of the social environment, brought together in terms of a fourth factor, that of development, for which he proposes a formulation in terms of contemporary equilibrium theory. With this general position I am in full accord; I believe the development of such a theory is practicable and that its expression by the use of contemporary mathematical models may be fruitful, although I reserve judgment as to whether the adoption of economic emphasis—calculations of strategy based on gains and losses—is the most rewarding model from amongst the available ones. This aspect of the problem is, however, the domain of General System Theory represented in our Group by Dr. von Bertalanffy, and I shall not address myself to it further.

However, throughout Professor Piaget's paper there appears from time to time a second and quite contrasting approach, in which the recognition of the importance of individual differences—as opposed to 'average' performance of individuals at a given 'stage'—the recognition of the role of the culture in advancing or retarding any of these assumed sequences, and the recognition of continuity rather than 'stages' in physical growth (p. 11)—are all treated, not as providing additional and needed material for a general theory of the development of the child, but rather as opposing theories or disproportionate emphases upon one of these three traditional factors. If this approach were followed it would be tantamount to saying that it is possible to establish stages if one confines oneself to the study of the cognitive and affective behaviour of children in twentieth century Euro-American culture, and leaves out of account material on their physical development, and material on children in other

cultures. As such an expectation contradicts the whole intention of Professor Piaget's integrating formulation, I merely mention it here, at the beginning, to stress that I am addressing myself to my alternative understanding of his paper, and not to the assumption that study of the factors of physical growth and culture automatically results in disagreement.

In regard to the question of 'general stages', our present cross-cultural evidence, admittedly very fragmentary, suggests that it becomes decreasingly possible to relate different aspects of the child's behaviour to its age or other measure of development, as age increases. The duration of development may nevertheless be of some significance in explaining different configurations of learning. I say 'duration' to allow for periods of illness or regression, or for extreme differences in the amount of interpersonal interaction in the life of a given child, who may, for example, be said to be equivalent—in this widest developmental sense—to a much younger or older child, because of the intervention of factors of acceleration or arrest of interpersonal contacts. For infants and very young children, the gross developmental conditions of learning to walk and talk also seem to introduce, in all known cross-cultural contexts, a certain degree of generalization into all other types of learning occurring at the same time. It must also be recognized that in regard to such things as walking and talking, different individual constitutions and different arrangements for learning in different cultures—as for example when children are kept swaddled or cradled beyond the period when they could walk, or hear phrases stated in their name long before they could formulate them themselves—may both vary to such an extent as again to make any idea of general stages appear useless.

If a theory of stages is conceived as a progressive series of equilibria, disequilibria and re-equilibria, in which successive equilibrium levels, even of only momentary duration, may be distinguished, but in which in any given process certain fixed sequences may occur, this formulation can be applied, with our present knowledge, to the investigation of human development within different cultures. As Professor Piaget now formulates the problem, such exploration would have to be done in very great detail, using tests which were formally identical and culturally comparable on a series of identified children, whose physical development had also been studied, over a sufficient period of time so that *transitions* might be examined and analysed in the case of these identified children. The question of the average age for the appearance of a stage while useful as a corrective for ethnocentric overgeneralization from studies on children of a particular culture, seems to me to be of only very limited significance. Only when the actual *succession* of stages in the development of any process can be followed in identified

individuals, in an identified culture, within an identified social unit (that is a group in which each individual's place in relation to each other individual is known) can we begin to relate together the three factors affecting development. I would maintain this as necessary because if, as Professor Piaget suggests, there are no general stages, then retardation or acceleration—(in terms of chronological age or developmental duration)—in any one process, attributable either to culture or to constitution, may have the most profound effects on the configuration of learning, and thus on the development of the personality. It may be that our most acute understanding of the constant sequences in any process may come in those processes where averaging is possible—a position which has not, I believe, yet been demonstrated fully—but that for the understanding of total development, it will still be necessary to take account of the effects of different combinations among these constant or fixed sequences, which are themselves systematically associated with genetic or with cultural patterns.

The specific study of identified individuals makes the distinction which Professor Piaget (p. 20) draws between *molar* phenomena and *molecular* phenomena less significant, for it makes it possible to address research immediately to the molecular level. When the general system of culture is examined as manifested in the behaviour of identified individuals in their interaction with a new member of the society—a formulation which permits more exact study than a formulation in terms of 'generations'—it is then possible to relate this behaviour not only to the whole system of culture (which may be expected to be sufficiently redundant to allow for the total genetic range of contemporary survival possibilities of *homo sapiens*), but also to the peculiarities of certain stocks within the society which have become isolated by various breeding barriers—such as class, cult, sect, occupational lines, etc. In this way it will be possible to investigate the degree of facilitation or inhibition existent, for example, in cultural systems which make a very slight use of mathematics, or in which the perception of time-space relations are very differently organized.

To address myself to the specific questions of Professor Piaget (p. 25, III):

I would not say (with Durkheim) that 'under all civilizations lies the civilization', but that all civilizations express the conditions of being human (*la condition humaine*), in that *homo sapiens* is dependent for his humanity—his survival as a species in the form we call human—on a system of socially transmitted learned behaviour. This learned behaviour shows certain regularities which can be related to the requirements of man's biological characteristics—long infancy, properties of the central nervous system, etc.—in combination with

the rest of the environment on this planet. Without such an assumption of regularities all comparative work between different cultures would obviously take a very different form, and such an assumption does underlie all contemporary work in cultural anthropology.

The way in which children learn natural languages may be regarded as a case in point. As far as is known children learn languages which on other grounds may be classified as easy or difficult, and of many different types of complexity, at the same age in all cultures. This can be attributed to two factors—the redundancy of natural languages and the fact that all first languages are learned in interaction between speakers and those who have not learned to speak at all, in the same way. The nature of speech, and the particular language spoken are communicated together. With the rationalization of the cultural understanding of languages, the development of such ideas as 'a language', 'grammar', 'word', 'alphabet', 'verb', 'predicate', 'utterance', 'phoneme', 'morpheme', the process of linguistic learning is becoming progressively transformed. So, in response to Professor Piaget's second question, if an adult had to learn a language as a child learns it, without the intervention of any categories of linguistic analysis, there would undoubtedly be found many similarities in the order of acquisition. However, in all known societies, a difference occurs because the idea of the existence of different languages has already been formalized, and while the child learns to speak, the adult, having learned to speak, learns to speak a second language. It is quite conceivable that the systematic and very early teaching of the alphabet and of reading and writing might introduce into the first learning of the child a new factor which would make learning language in complex, fully literate societies no longer comparable with learning languages in non-literate or slightly literate societies, and that some effects of this sort may be making themselves felt in the present difficulties which are being encountered in efforts to give a type of early education designed for children of the literate to children of the non-literate.

However—still in response to Professor Piaget's second question—it would appear that every cultural system contains within it the provision for the way in which it must be learned by children during their normal development, including sufficient leeway for a range in this normal development—in such respects as type of imagery, capacity to organize, type of memory, etc. One route to a comprehension of another culture, or some complex part of a culture, such as the language, the legal system, the ritual idiom, etc., is to repeat the steps taken by children learning this system. This contrasts with the way in which a linguistically sophisticated adult masters the 'grammar' of another language in a matter of hours, or a mathematically sophisticated adult masters a new type of mathematics. It would seem

that once having traversed the steps necessary to become human, in any culture, one may transfer that learning, at an adult level, to any other culture, but that cultures differ in the developmental levels which they call into play in certain areas of experience. So western culture has now developed to a high degree the type of thinking necessary for scientific endeavour, but leaves in a quite uncultivated state various capacities for introspective experience developed in Indian culture. For members of cultures which have not elaborated our type of scientific thinking, immediate transference of previous learning into understanding of our culture may be possible only for the exceptionally gifted as it may mean an imaginative act of transference covering a whole series of missing stages, in the form in which they have been culturally elaborated in our own society. For the less gifted, it may be necessary to include in any education in another culture a re-experience of earlier stages of learning, in the different cultural form. As I have understood Professor Piaget's discussions in our meetings this formulation is one which he feels is compatible with his material.

In conclusion, I should like to stress the significance of our failure, as a group, to pin down our various conceptualizations in any single set of individually identified real children. The insights which each of us has brought to these four rewarding years of work have all been based on very careful precise observations made, over time, on individual children, or in Konrad Lorenz's case, other living creatures in known social contexts. However much we may have concentrated on cognitive behaviour, or emotional disturbance, on behaviour as part of a group in a primitive culture, on electroencephalograms or physical measurements of growth, much of the background of the children studied, or of the other aspects of the children's development was always known, and always taken, however inexplicitly, into account. Not until this new integration proposed by Professor Piaget and modified by the more formal inclusion of differences among children, and among cultures, can be applied to the detailed simultaneous study of a group of *children*, will we know what we have attained in this new way of looking at the psychobiological development of the child. And as I wrote the last sentence I realized that I should lament this, but not reproach ourselves for it, for our mandate was, after all, only to think about the child, to present an integrated set of abstractions which, one hopes, may be used in the study of children in many different parts of the world.

## Comments on Professor Piaget's Paper

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In replying to Professor Piaget's paper from the standpoint of a neurophysiologist, I must begin by saying that, like the other neurophysiologists of our group, I am eager to see the organism whole and to avoid organ dogma. I think it is as silly to attribute all behaviour to the cerebral cortex (or to the reticular system, which is more fashionable) as it is to the possession or not of a penis. Of course we know that we can never see an organism and its history truly whole, but we must try and try to get a glimpse in 3D if not in the 7D we know we need. Opposite p. 148 of our first Proceedings (Vol. I) is an old record showing the simultaneous recording of ten variables during a psychological experiment. We cannot control the variables when we are studying a complex system, so we must observe as many as we can and make all illuminate all. This is the essence of the cybernetic approach. There are still shadows and highlights and deep ravines of inspissated gloom, but we are beginning to see the modelling of the growing brain in its relation to the rest of the body and to behaviour. As a result, the answer I must give to Piaget's questions will be contingent rather than concise. For one thing we are perpetually impressed by the range of variation between children in a given age group—yet Piaget speaks for example of '*the child aged 7-8 years*'. Of course we can accept a *statistical* norm for some of our physiological variables, but if, as we think likely, development of some variables proceeds by steps, then their time-distribution is not normal in the statistical sense and we must specify whether in a particular child a certain step has been made or not. Paediatricians are always talking of 'milestones' in a child's development, but this gives an impression of an arbitrary scale at the side of a smooth road—what I am concerned with is an actual change of plane or field or climate, where, as in the dark wood of middle life, the straight road may seem to lose itself in the undergrowth, and we have to take to the trees.

The question of use and design, or nurture and nature, and the notion of equilibration are affected by this concept, for implicit in it is the consideration of the organism and its environment as a

closely interacting *statistical* assembly. Perhaps I mean rather a probabilistic matrix, or a quantum atmosphere; Bertalanffy includes such systems in his General System Theory and Ashby calls certain classes 'ultrastable'. We are in great semantic difficulty here. I don't fully understand Ashby's equations. I can't write down any better ones and the grandiloquence and ambiguity of the above phrases appals me. Furthermore, I have to postulate what I have called 'speculation' in living systems. My models show what I mean if you can remember them; think of all their components assembled together, receptors, relays, storage systems, motors and all, then imagine wires growing out from one component to the other along the potential gradients, so that at first there are only a few simple reflexes, then the scanner starts up the 'speculative' activity, an IRM is triggered, the probability of significance of some sets of events reaches the threshold of implication, the IRM is incorporated into the new association, the original reflexes are adapted to a more complex application. All this time you could amuse yourself by betting on what would happen next and you might win a little if you knew what was inside the boxes, but the only certain thing is that in the end the batteries would run down; the system would no longer be 'open' but very definitely closed, dead.

Now, if you had been playing with the model since it was 'born' and knew what was inside it, you could give a fairly good account of its state and, as I say, a fair prediction of its next phase. But if you came on it after it had already gone through a few manoeuvres you would be quite unable to describe its internal condition. Obviously you could construct an objective description of its behaviour and if you were allowed to use a few instruments you could also identify some of its internal functions; perhaps relate function to behaviour in a tentative way. But you could not *by any means* discriminate between built-in and acquired features. This is a corollary of the proposition so carefully set out by Wiener in *Cybernetics* about biological or Bergsonian time as compared with Newtonian time. We exist on an irreversible time-scale; we cannot live backwards and cannot even make legitimate retrospective analyses without inside information. This is nothing to do with any mystical properties of Time but is because what we see of living creatures—and models of them—are statistically determined interactions between structure and experience. What we call logical processes, the rules of arithmetic and of games, are very special cases, artificially isolated and enforced for special purposes. It seems to me one of the limitations of the Freudian way of thinking that it assumes a principle of strict logical causality in mental processes.

I suppose Adler is dreadfully outmoded but I have always admired his emphasis on the personal vector. The assumption of orientation

need not be coloured with transcendental teleology in thinking about children any more than in watching my models. Artificial goal-seeking mechanisms are novel perhaps, but the essence of cybernetics is to define and analyse the factors in 'purposeful' behaviour which are common to all self-controlled systems, and to suggest tactics and strategy for the study of complex interacting systems—which are beyond the range of classical scientific methods and propositional logic.

What seems to me very important in all our discussions is to recognize that complex behavioural patterns which seem to show intense purposefulness may be the expression of quite simple mechanisms. The elements in the mechanism need not be very numerous either, but of course the number of ways in which they can be combined in permutation is certainly colossal, and as Bowlby mentions, very slight differences in their rate of development or in the details of their connectedness will be grossly amplified by the very operation of the mechanism itself on the environment and vice versa, so that again great differences between young individuals of the same age are to be expected and should be appreciated. Furthermore, these very differences should give us essential clues to the whole problem of animal development, since the permissible range, or 'tolerances' as an engineer would call them, of a component or function often reveals far more clearly than its mean or modal value, what part it plays in the whole organism.

When Ross Ashby asked us to help him build one of his homeostatic analogues, he gave us a circuit diagram with the values of the resistances and voltages and so on. Several of these were unusual values and rather hard to obtain. Our electronic craftsman looked the circuits over and said, 'I'm awfully sorry but I can't get that value of resistance, 63,258 ohms, very easily—would 68,000 ohms do instead?' Ashby replied, 'Oh, yes, it doesn't really matter what the value is—I just measured the resistances I happened to put in and that is the average value—they can be almost anything provided they don't pass too much current.' The craftsman looked as though he had found something slimy under a large stone, and we had to subcontract the job. His reaction was perfectly apt—such a specification is too much like life to please a mechanic who wants his machine to behave exactly according to plan. I should add, to point the moral, that Ashby also specified that certain values, of supply voltage for example, should be very carefully fixed and stabilized. This seems to me very important; in complex systems which are capable of self-control and self-development, whether in flesh or metal, some features must be held constant within narrow limits, others can be—in fact must be—left free to vary widely. In other words, in our study of development we might usefully try to decide

which aspects of human psychophysiology are homeostatically controlled and which are liable to vary over a wide range. As a simple example, body temperature varies little (as compared with ambient temperature). We know something of the reflexive or 'negative feedback' pathways that ensure this in health and use the failures of control as an aid to the diagnosis of faults or diseases in the organism. On the other hand, EEG characters (which can be measured with a comparable degree of accuracy) vary over a wide range and this variation makes the diagnosis of brain disorders in children by EEG enormously difficult. Are these two classes of relationship what Ashby means by 'Parameters' and 'Variables'? (A parameter being a variable which is not included in a system under consideration.) This is by no means trivial or academic for, to quote Ashby, 'a change of parameter-value changes the field, and because a system's stability depends on its field, a change of parameter value will in general change a system's stability in some way.'

These notions seem to me very apposite to the discussion by Piaget and Bowlby on 'equilibrium'. I feel bound to add that in our studies of human brains we have quite independently been forced to include 'stability' as a measurable neurophysiological relationship, among our *parameters*, and feel justified in identifying certain mechanisms in the brain as serving to ensure failure-to-safety in conditions where stability is threatened by a change in Parameter-value A. I shall be discussing this in more detail in my replies to Piaget's specific questions to me.

This is saying the same thing as others have suggested about whether a child tends to reach an equilibrium (Piaget), or a steady state (Bertalanffy), or stability (Ashby) or normality, or whether in general organisms are essentially spontaneously active (Bertalanffy) or speculative (GW). I am suggesting that there are indeed reflexive mechanisms (a phrase I prefer to 'negative feedback circuits') which are intrinsically homeostatic and self-stabilizing, error-cancelling. Further, I agree with Bertalanffy that the signals or events or stimuli which operate these mechanisms are in general (and particularly in humans) of such variety and intensity that it is better to think of the outcome as the steady state of an open system. As Bertalanffy says, this is classical but not trivial and an important corollary is that when one studies the internal economy of an open-system-in-a-steady-state one often finds a surprising amount of activity going on because the channels carrying the error-signals are likely to be pretty busy even when the whole organism seems in 'equilibrium'. In fact, the more nearly perfect the 'dynamic equilibrium' the more internal 'activity' there may be. There are plenty of examples of this, of course, in flesh and metal.

A very crude illustration for those who haven't thought in these

terms: the humble and hygienic water-closet problem. You want a tank of water to be always full so that it will operate a siphon. You can have a continuous inflow with an overflow pipe—very simple, no moving parts except the water. But very wasteful, and the faster you want the tank to fill after it had been emptied, the more wasteful it will be. This is a closed system in a 'dynamic equilibrium' of the simplest type, and is adequate if the tank is likely to be emptied on the average almost as soon as it is full. To avoid waste, however, you fit a ball-cock which admits water in inverse proportion to the existing water level. Now there is no waste at all, and the rate of filling can be as fast as the supply pressure and capacity permit. But you have a more elaborate reflex mechanism which might go wrong and if you are wise you will leave the overflow pipe too, just in case, to ensure failure-to-safety.

Now note how, from outside the tank, you could distinguish the first system from the second. In the first place there would be steady 'spontaneous' activity in the input and there would be two alternative outputs, the siphon or the overflow, related in such a way that when the first was operated the second was inhibited and the first would only operate if the second was already working. In the second system on the other hand the input would only appear *after* the siphon was operated and then for a fixed time, during which the siphon would be inhibited. To take the analogy a step further, if you decide that the whole system should be automatic in the sense of not requiring an initiating stimulus, you could either arrange for an inverted U-tube siphon (which would empty the tank quickly and completely whenever it was full, the frequency of discharge being a linear function of the input rate), or add to the reflex system a clock which would initiate the sequence of siphoning and filling at regular intervals. This last would ensure relative independence of filling rate and supply pressure. It could also be linked with a receptor to provide information about when the discharge would be most effective, thus forming a second-order reflex, with further possibilities of devices for estimating contingency between apparently independent events, conditional probability, and so forth. In these more sophisticated arrangements, inspection would reveal 'spontaneous rhythmic activity' which might be significantly related in its phase relation to the pattern of incoming signals, but would show little connexion with the characters of the energy supply. Above all, the more perfect and intricate the dynamic balance, the more characteristic and varied would be the internal spontaneous activity. (Obviously this word 'spontaneous' is an awkward one. This is no place to discuss it in detail. Many other terms have been used: 'autochthonous', 'autogenous', 'autogenic' and so forth.)

We are still debating whether in the brain small populations of

cells or individual cells do really exhibit gratuitous, autogenous activity. In any case, whether an isolated cell, or a slab of cortex or a whole brain or organism is considered, wherever rhythmic or repetitive activity is observed, there must be a 'feedback' of some sort. This is true even of a pendulum; so it is not surprising that we find such effects in profusion in animals or that they are often inversely related to outward function or operational activity, for as in the case of our water tank in the household they are usually essentially administrative rather than operational.

This bring us back to the basic question: can we hope to distinguish in ourselves, in children, between on the one hand the administrative homeostatic mechanisms which, from the point of view of mentality and behaviour are concerned with parameters (A) and on the other the operational, speculative processes, the manipulation of essential variables, which gives us such deep satisfaction—happiness as opposed to comfort?

Now I must reply more explicitly to Piaget's questions to me.

The first question dealt with the possible forms of psychobiological development, of which I suggested six. Piaget proposed another to describe the construction of logical relationships, which he feels is an important stage at 7-8 years, and he describes this as *Learning by successive equilibrations*. For me this presents few problems since I consider such learning as a *special case* of 'stochastic' learning just as I consider logical reasoning as a special case of statistical reflexion. I agree of course that propositions such as  $A=B$ ,  $B=C$ , therefore  $A=C$  are unlikely to be worked out by children under 7-8, but I think of this class of propositions as a member of the (larger) class: 'A usually implies B, B usually implies C, so there is a reasonable chance that A will imply C.' Remember that in such a chain of *probabilities* the uncertainty is multiplied at each stage, so that the chance of an organism making the *logical* inference is quite small unless the probability is near unity on every occasion and at each stage. In the example Piaget gave of his friend counting pebbles (Vol. II, p. 59), he counted them several times, changing the order and making sure that the cardinal sum was invariant with order. Piaget's point was that the organism here was actually manipulating the environment, and I agree that this is of basic importance. But I maintain that in the development of an assembly of statistical associations a stage will *sometimes* be reached at which a completely invariant component will emerge and this may become a basis for logical reasoning. This is really quite familiar, and is sometimes described as the search for redundancy; for example, in the question of cardinal number and ordinal number, the pebbles or the fingers are found to be 'redundant'. The relations sometimes described as Natural Laws are in fact examples of enormous redundancy. In general, the degree

of redundancy, the completeness of invariance of Natural Laws, cannot be determined without experimental verification, that is, physiological action by the organism. There is only a very poor chance of the signals received passively by an organism providing enough information for it to draw a general conclusion. Hence the need for 'speculation', the active and assiduous exploration of promising relations; in scientific work the experimental testing of plausible hypotheses. This is included in my class of Learning by Association since a large proportion of the relevant information received by the brain is from proprioceptors responding to muscular and glandular action, rather than from exteroceptors.

It seems to me that the 'learning par équilibrations successives' of Piaget would be better translated as 'matching' (a word which cannot be translated into French; it is used often in technical French in phrases such as 'Pour assurer un minimum de distorsion dynamique il faut que les selfs soient matchés . . .': the inductances must be matched). This is a most important concept, but is not outside the mechano-physiological classification I put forward. Learning by matching obviously implies the existence within the organism of some sort of model to match with experience and I suggest that a child is *unlikely* to acquire enough information to build up in his brain a stock of 'logical' models before the age of 7 or so.

This is just the sort of question I had hoped to be able to answer by experiment. We have evidence—in reply to Piaget's second question to me—that elaboration of sensory signals is rare in children below the age of 7 or so. Is this because elaboration depends upon progressive matching of internal models with external experience and this cannot be done until there has been time for a stock of internal models to be built up, or is it because the neural apparatus is not there at all before this age? This should be a straightforward problem and seems to me a basic one, but it cannot be answered, I fear, without experiments of a fairly intricate nature.

This leads on to my answer to Piaget's third question; why is there so thin a correspondence between EEG and cognitive development? I think this is true only if one is limited to subjective consideration of the passive EEG features. If one makes quantitative studies of the EEG and other physiological variables during activity, particularly during a learning experiment, very exciting relationships emerge which do indeed seem to encourage the search for a firm passage between physio- and psychological domains. Such experiments are, alas, still very involved and expensive, but I don't feel anything simpler is worth doing. Some of you may well feel that our claims are exaggerated and that we make too much technical fuss—you may be right, but the old method of taking a passive EEG and glancing through the record is rather as though Tanner were

to glance at one of his child subjects and say vaguely, 'Yes, that's a well developed child.' May I give another analogy—suppose we were trying to decide how to teach children gymnastics and the question arose, at what age should we expect a particular child to be able to raise itself up to a horizontal bar? Would it be enough to measure the girth of the upper arm at rest? Would we not have to consider practice, incentive, competition, physical proportionality? Our psychophysiological experiments are rather like that; we are trying to find out how and when people become capable of performing certain mental gymnastics. At the present stage it looks as though there are definite turning points in development, when certain mechanisms reach a threshold of operational efficiency and begin to have external effects. I must stress again the serious difficulties this raises—classical methods of observation and analysis do not allow readily for non-linear or threshold effects. The popular psychological notion of 'insight' is one of these. In my model (Vol. II, Fig. 11, pp. 32-33) there is a component labelled 'insight' which indicates when the 'experience' of the model is adequate to justify external action. Mechanically it is a threshold device which does nothing until the stress on it reaches a certain value when it 'fires' and transmits a single 'bit' (binary digit) of information to a storage register. This is an abrupt event which apparently—seen from outside—has no precedent; that is to say, nothing seems to be happening for a long time, then suddenly the whole situation is changed. Actually, within the box plenty is happening but it is not reflected in action. There is nothing mysterious in this of course, but it is important to realize that the link between a great deal of selective activity at the input and a quite elaborate novel action at the output is just a single 'bit' of information which tells the storage system that a certain degree of improbability has been surpassed. This relation is very hard to put into words, and beyond my capacity to condense into algebra, which is why I made the model; when met with in a living organism it is terribly confusing and impressive.

That is why the existence of step functions or thresholds in physical development *apart* from behavioural effects such as 'insight' must be investigated very carefully; if a system contains the two together the problem may be very nearly intractable. None of us is really trained to handle this sort of problem—it is hard to preserve one's dignity and poise going upstairs on skis. I say again—mind the step!

## Comments on Professor Piaget's Paper

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Two of the main questions raised by Piaget concern me closely: the existence of stages of development, and the mechanism of transformation from one stage to another. The first of these questions I think largely spurious; that is I consider it chiefly a matter of the different use of words by different members of the group. Here I am in total agreement with the remarks made by Lorenz on this subject at the third meeting (Vol. III, p. 162). An example below will, I hope, make my standpoint clear. It will show, amongst other things, that I assume Piaget's conditions for existence of a stage numbers 1, 2, 3 and possibly 4 (pp. 13-14) as existing all the time throughout growth and governing continually the physical growth of the organism from moment to moment. I would not myself consider these conditions sufficient warrant for the use of the word 'stage'.

The second question, that of the mechanism of transformation, I consider the most fundamental question in our whole field and probably the most important. I am only sorry that I can contribute practically nothing to its elucidation, whereas as a physiologist I might be expected to contribute perhaps the most. The fact is that physiological ignorance on this matter is profound, and professional physiological interest and experiment almost non-existent.

Dealing now with the specific questions addressed to me:

1 (and 3). The *immediate* cause of the chief phase of acceleration, that at adolescence, is well known. The spurt in bodily growth is caused by the release of hormones from the gonads, adrenals and pituitary. The cause of the increase in gonad and adrenal secretions is the release of other pituitary hormones, and the release of these pituitary hormones is caused by certain events, the nature of which is obscure, in the C.N.S.

The most we can say about this at present is that at some stage of development some areas in the C.N.S. reach a state of maturity X, and as soon as X is reached, messages pass to the pituitary to release hormones previously stored there, and adolescence ensues. But what causes state X to appear? This question is in my view the fundamental one in all the study of physical growth.

We can sum up all we know about X by saying:

- (i) X does not depend much on the size of the organism; it depends more on the percentage of ultimate adult size achieved, i.e. X appears more nearly at say 80 per cent final size than at a size of so many grams.
- (ii) If the organism is starved and growth retarded, X is retarded. If the organism is given food after a length of time spent at constant weight, then X appears at about the same percentage of adult weight reached, quite irrespective of how many days have passed. The appearance of X is related to *internal maturational time*, not to chronological time.
- (iii) The moment when X is reached can be best predicted from observing the previous course of growth. It will then be seen that X occurs chronologically early in organisms all of whose progress-to-maturity has been swift or 'advanced'. That is, children with a bone age of 11 at chronological age 9 reach X earlier than children with a bone age of 9 at chronological age 9.

This leads one to suppose that the stage X proceeds from state W, which in turn proceeds from state V, etc. One can think of growth and development as a continuous series of states  $\rightarrow U \rightarrow V \rightarrow W \rightarrow X \rightarrow$ : the organism may be temporarily arrested in any of the states, but the *order* in which they occur is always the same. One cannot proceed, for example, from U to W except through V, unless some pathological disturbance supervenes.

This formulation lends itself to a symbolization of what I believe to be the relation between those who use the concept of stages (Piaget for cognition) and those who prefer the concept of 'continuous' development (Piaget for perception, myself for physical growth). If we symbolize *observable* change by length of arrow and state by length of dotted line associated with the capital letter, we have:  
Stages:  $\rightarrow \dots U \dots \rightarrow \dots V \dots \rightarrow \dots W \dots \rightarrow$   
Continuous:  $\text{——} \rightarrow U \text{——} \rightarrow V \text{——} \rightarrow W \text{——} \rightarrow$   
In part, I feel sure, the idea of 'stages' arises from an inability to measure small increments of function; but in part the idea may truthfully reflect a situation where in one part of the organism no change is occurring (while continuous growth meanwhile goes on in another part).

As to the mechanism of these  $U \rightarrow V \rightarrow W \rightarrow$ , and the question of equilibria: we know very little about this and I can only repeat what I said at our first meeting—that the process of maturation seems to me like a series of clocks, the signal for the starting of one being the running-down of another. I would now complicate this a little by adding that there are many clocks all going at once, and clock B starts when A reaches four o'clock, clock C when A reaches six o'clock, and so on.

Undoubtedly there are feed-back mechanisms wholly within the C.N.S. governing the  $U \rightarrow V \rightarrow W$  mechanism. Whether there are feed-backs of the sort C.N.S.  $\rightarrow$  endocrines  $\rightarrow$  C.N.S. or C.N.S.  $\rightarrow$  endocrines  $\rightarrow$  peripheral tissues  $\rightarrow$  C.N.S. we do not know. (Equally we have no really clear idea of how far environmental stimulation, e.g. by social conditioning or simple physical exercise, can enter the feed-back circuit.) We do know of various substances (both oestrogens and androgens) which will speed up maturation of particular bits of the organism—for example the ossification of the bones at the wrist, or the appearance of pubic hair—but we do not know of any substances or any treatment (except starvation) which will speed up or slow down the rate of development of the organism as a whole, that is, while maintaining its normally balanced structure.

2. Our knowledge of anatomical and histological changes in the human brain after the age of six months is virtually nil. Probably by the age of nine months, and almost certainly by one year, all the cells and all the fibres of the C.N.S. are present, and all the fibres ultimately to receive myelin have begun to become myelinated. After this time, however, the diameters of the myelin sheaths increase, and probably also the size of some nerve cells increases. About detailed histological changes at the periods 1½-2; 7-8; 11-12, we have no information whatever. I leave the question of EEG evidence for functional changes to Grey Walter to answer: apart from the EEG I do not think there are any useful physiological data on C.N.S. function during this time.

Comments on Professor Piaget's Paper

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Professor Piaget's essay on the general problems of the psycho-biological development of the child has given me once more the opportunity to take up the thread of our discussions.

1. *The concept of interaction between factors affecting development*

Partitioning into compartments is dangerous, but it is necessary to distinguish between factors. This second proposition must be stated as clearly as the first if we are to avoid falling from atomistic error into a confused globalism.

Moreover the interaction Piaget mentions presupposes the existence of factors. Of course, it must be pointed out that these factors do not act as independent variables. It is, for example, an organism which 'chooses' and organizes its environment in accordance with its heredity, and in turn this environment acts on the expression of the heredity, and perhaps eventually on its transformation. I agree here with Piaget, and the only fault I would find in his formulation would be undue caution. He says 'The interactions (of factors) are at least as important as their respective actions.' It seems to me that there are *no* isolated actions. The action of a factor is accomplished and can be analysed only through interactions even if, in extreme cases, this action of a factor strongly dominates the others.

Wallon and I tend to trace everything back to the maturation of the nervous system and to the social factor. Two very important matters must be made quite clear however:

(a) I do not consider these factors of maturation and environment to be additive. They determine each other in a progressive integration. Moreover the conception of the social factor runs the risk of creating misunderstanding if by it one means only the general framework within which the individual's activity is carried out. The conception of syncretic sociability defined by Wallon at the stage of pure emotivity implies a very archaic level of the 'social' and at the same time stresses the original interdependence of the organism and the social. I personally have used the method of twin investigation more

in order to study the dialectics of inter-individual relations than to solve the classical problem of the relative importance of Heredity and Environment (which seems to me a very ambiguous problem in any case). This corresponds with what Piaget (p. 20)—using a debatable term—calls molecular phenomena of social life. The term is debatable in my opinion because the idea of the molecule may lead us to think that social life in the 'molar' sense (general form of society) is only the combination of individual interactions.

(b) Mind ('psychism') cannot be reduced to factors. The human being is not reduced to the conditions of his existence. A new reality arises from the processes of integration of the different factors. Through his actions and his conscience (whatever the definition given to conscience) the individual becomes in turn the *agent* of his development and his behaviour. Although I am reluctant to admit that equilibrium is a factor, I nevertheless consider that, in the dynamics of development, effects become in turn causes. Here we reach the problem of conscience, of interiorization, and of autonomy, which scientific psychology cannot solve by denial.

## 2. *Concept of equilibrium*

The phenomena of equilibrium and regulation are incontrovertible. But I am not quite sure:

- (a) of understanding exactly the explanatory significance Piaget gives to the conception of equilibrium considered as a factor;
- (b) of agreeing with him as to the wide extent which he attributes to the phenomenon of equilibrium in all natural fields—physical, psychological and sociological.

First of all I find it rather difficult to admit under the same category of factors heredity, environment, and equilibrium. It seems to me that equilibrium is always a relationship, a *law* of organization between elements or between various causal series. If one classes equilibrium as a fourth factor alongside material causes and conditions one runs the risk either of *substantializing*, of hypostasizing the laws of equilibrium, or else of *dematerializing* the material factors of development, making them disappear in a pure game of intemporal relations, a mental algebra.

Moreover, I was very much struck by the fact that the concrete examples Piaget gives of the *independence* of the equilibrium 'factor' are in relation to the solution of a problem at a given stage of development and not to an actual *genesis*: as if what was essential was the equilibrium, a sort of final cause, transcending the conditions of development. Here he stresses *that which does not change* (the eternal and general laws of logic), and does not consider, as such, *that which changes*.

In any case it seems to me necessary to distinguish between the process of equilibrium *during genesis* (an equilibrium established between well-defined material conditions) and the process of equilibrium as the *search for a solution*.

### 3. *The concept of a common language*

The scientific mind requires, of course, that the same things should be said in the same terms and that the same term should be applied exclusively to the same thing. Specializations are accompanied by conceptual organizations and jargons which are often undecipherable from one specialization to another.

However, one may sometimes be led to believe that the idea of a common language and the isomorphism of the different levels of reality are interdependent, for example if one speaks of the 'translation from one viewpoint into another'. In the case of the EEG, intellectual operations and social relations, their common laws of strategy, economy and equilibrium might authorize a common language and, of course, the more clearly the identity of the laws emerges from the diversity of the phenomena, the easier would it be to establish a common language. Moreover, scientists have a strong tendency to attempt this reduction of reality, this 'identification', as Emil Meyerson has shown so well in his famous thesis.

But we cannot postulate what still remains to be demonstrated and consequently we must not base a common language on what is only a heuristic attitude or a working hypothesis. In short, if 'common language' signifies use of common terms to designate the same things, then I am in agreement with Piaget; if 'common language' is the expression of a postulate of isomorphism, then I am not.

Certainly our various languages must become inter-coherent in their attempt at expressing the coherence of the various levels of reality. We must look for analogies, parallelisms and common laws and also, within the same field and from one field to another, relations between cause and effect where the effect cannot be reduced to its cause.

Practically speaking, the search for a common language is not just limited to the search for common terms in special scientific fields, but includes the statement of clear definitions, that is to say definitions communicable without ambiguity outside the narrow circle of a speciality.

### 4. *The concept of stages of development*

I do not think I have understood very well Piaget's criteria of the concept of stage. However, I have here two preliminary remarks to make:

(a) I am not certain that his definition applies to everything that might be considered as a stage. He has defined the concept of stage on lines suggested by the study of intelligence. If one demands criteria—particularly the characteristic of structural equilibrium—which are perhaps peculiar to cognitive functions, one runs the risk of neglecting in other fields the existence of stages which would be expressed only by a constant order of succession and by integration. There is a risk of ending up for example with the syllogism which would sterilize all scientific initiative: the stage is an equilibrium structure, therefore there are no stages in the development of personality.

In the present state of our knowledge it seems to me more profitable to agree upon a much less restrictive definition, having as criteria only *constant order* and *integration*.

(b) I am not certain that I have correctly understood Piaget's attitude concerning the evolution of non-cognitive functions. Sometimes it seems to me that he is sceptical about the existence of stages other than intellectual stages: in particular, he mentions on several occasions that the cognitive factors correspond to the structure of behaviour whereas the affective factors correspond only to their energy component.

##### 5. *Mechanisms of passage from one stage to another*

We must certainly come to an agreement as to what we mean by the word stage. But that is not enough. We must be fully aware of, and state clearly if necessary, the concepts which take account of the mechanisms and the dialectic of change: concepts of threshold, passage from a quantitative growth to a qualitative transformation, etc.

I should like therefore to come back to some commonplace affirmations which have been neglected during our discussions on the existence of stages:

1. The continuity of a growth (level of calcification, length of limb, increase in angle measuring muscular extensibility, degrees of myelinization) does not exclude *a priori* a transformation or a *discontinuity* on a functional level.
2. At a higher level of complexity the continuity of growth of physiological components or conditions can cause the appearance of stages at the psychological level. Thus we must look for the coherence of development rather in causal liaisons than in the correspondence of stages, whether or not the development occurs in successive levels.
3. The psycho-physiological evolution of the individual leads him into increasingly complex and extensive *environments* which can act as external organizers and thus cause stages.

4. It is true that the statement of dominant characters is frequently arbitrary. However, we must distinguish between a *subjectivity* which brings with it no proof, and an entirely legitimate *relativity* of points of view. The stages can vary according to the codes one uses for deciphering, but obviously each code must be clearly defined.

5. The hostility of many authors towards a denial of the conception of stages frequently arises from the desire to preserve whatever might be qualitative in the transformation of the individual, whether in psychology or physiology. It is necessary to stress strongly that a *qualitative transformation* (of the personality, for example) *can well be conceived without discontinuity*: a gradual transformation, as in light spectra. In this respect the concept of crisis remains to be clearly stated.

## Comments on Professor Piaget's Paper

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As I am a newcomer to the Group, the present memorandum is intended to give some idea as to the contribution I may be able to make to the discussion at the Fourth Meeting. I am basing my remarks on Professor Piaget's paper.

### I. *The quest for a common language*

Professor Piaget has admirably emphasized that, in order to arrive at some co-ordination and synthesis of various fields, a principal problem is that of a common language which, so to speak, is translatable from one field to the other.

1. I believe I can make a suggestion in this respect. In the last few years, a development has taken place which seems to correspond well to Professor Piaget's quest. It is the development of General System Theory (G.S.T.).

G.S.T. is intended to elaborate such principles as apply to 'systems' in general, irrespective of their particular kind, the nature of their component elements, and the relations or 'forces' between them. It thus provides a superstructure of theory generalized in comparison with the conventional fields of science. It is capable of giving exact definitions to many notions, such as, for example, wholeness, interaction, progressive differentiation, mechanization, centralization, dynamic and homeostatic (feedback) regulations, teleological behaviour, etc., which recur in all biological, behavioural, and social fields, have had some vitalistic or mystical flavour, and were not accounted for in the so-called 'mechanistic' approach. In such a way, G.S.T. accounts for the isomorphy of theoretical constructs and of the corresponding traits of reality in the diverse fields of science.

G.S.T. has been rather extensively applied in various fields during recent years, and it may be mentioned that a *Society for the Advancement of General Systems Theory*, which is a group within the *American Association for the Advancement of Science*, attempts further development and application of this field. The principal

goals of G.S.T. are: (a) in trying to integrate individual branches of science in their general principles; and (b) in offering a theoretical structure and models to those fields of science—especially the behavioural sciences—which still lack them.

Ashby's formulations (1952) referred to in previous meetings are closely related to those mentioned above. Even though he does not use the term General System Theory, he starts with the same mathematical model as I do. As Ashby and I have drawn different derivations from the same model, our work is complementary.

2. I am not going to review in this memorandum what has been done in this line as it may be found in the literature (Bertalanffy 1949, 1950a). Rather, I would like to give some clarification as to the bearing as well as the limitations of G.S.T. and interdisciplinary constructs in general. What will be said applies equally to other attempts at a 'common language', such as information theory, game theory, decision theory, cybernetics, and so forth.

Being an experimentalist with strong leanings towards physics myself, I am vividly aware of a danger apparent in much of current literature in the behavioural fields. Nothing is accomplished by loosely applying to unexplained or unco-ordinated facts some fashionable term—be it 'system', 'homeostasis', 'feedback', 'information', 'minimax solution', or whatever the case may be. Attaching some new verbal label must not be mistaken for a new insight or understanding.

What G.S.T. (and related constructs) can do is what Hayek (1955) has aptly discussed as 'explanation of the principle'. In physics (and to a certain extent other fields such as biophysics, genetics, etc.), there is a hypothetico-deductive system of laws, the appropriate parameters of which can be inserted; so we have explanation and prediction of individual empirical phenomena, such as the positions of the planets at any time in the past or future, the behaviour of atoms, or the result of some cross in *Drosophila*. Many biological and most behavioural phenomena (except such rather trivial aspects as e.g. mortality statistics) are too complicated and obscure in their structure to allow for such explanation and prediction. The best we can do—at least at present—is some 'explanation in principle'. What this means can best be illustrated by a few examples.

There is a highly elaborate mathematical theory of population, both from the ecological (Volterra, D'Ancona, Gause and others) and genetical (Fisher, Sewall Wright, etc.) viewpoints. All biologists agree that this theory provides an important basis for understanding the struggle for existence, biological equilibria, etc., and evolutionary processes, respectively. It is difficult, however, to prove quantitatively, say, Volterra's laws of population growth even in laboratory experiments, and it is nearly impossible to do so in the field as the complexity of natural ecological and genetical systems prohibits giving

concrete values to the relevant parameters (coefficients of reproduction, extinction, and competition; selective advantage, coefficients for drift and the like).

Economics and econometrics provide theoretical models which are more or less generally accepted. As a rule, however, professors of economics are not millionaires, showing that though they can give 'explanations in principle' for the economic process, they are not in a position to predict the fluctuations of the market with respect to a definite date or an individual stock.

Game theory (referred to as a possible model by Piaget, at p. 7) is a novel and original mathematical field. However, I understand from competent authorities that hardly any examples except trivial ones can be figured out specifically in the way a physicist or engineer would calculate a phenomenon or a machine, even though the theory may provide explanations 'in principle' for psychological and social phenomena.

Similar considerations apply to G.S.T. It is in a position to offer 'explanation in principle'; but it cannot be blamed for not giving quantitative solutions for phenomena like embryonic regulation, psychobiological development, etc., where the complexity of the process and the lack of definition of the relevant parameters are prohibitive.

So much about G.S.T. and theoretical models in general. What more can it offer for the problems of psychobiological development?

## II. *The question of an equilibrium factor*

1. Professor Piaget suggests that, besides the factors customarily envisaged in development, another principle should be considered which he calls the 'equilibrium factor'.

In principle, I am in full agreement with Professor Piaget, but I believe that this viewpoint can be considerably improved if, instead of the notion of 'equilibrium', somewhat different conceptions are taken as the starting point.

Looking first at the organism from the physiological viewpoint, it is a basic characteristic that it is not a system in equilibrium. On the contrary, for a system to be living presupposes that it avoids the state of equilibrium. If equilibrium—chemical, osmotic, thermodynamic, etc.—is reached, this means death.

This avoidance of equilibrium is possible because the organism is an open system, maintaining itself in continuous exchange, building up and breaking down its components. An open system and an organism may reach a time-independent state where it appears to remain macroscopically unchanged; but this is not 'equilibrium' but a 'steady state'.

Although this seems to be trivial and the living organism has been called a 'dynamic equilibrium' for a long time, only in recent years has the theory of open system and steady states—kinetic and thermodynamic—been developed. The laws governing open systems and steady states are characteristically different from those governing the conventional closed systems and equilibria. Again I have to forsake a more detailed explanation and refer to current literature (for a general orientation: cf. Bertalanffy, 1950b, 1953a, 1954; Bray & White, 1954; Jung, 1956). I am taking up only a few aspects which may be important in relation to problems of behaviour.

It is a basic tenet in biology and psychobiology that the organism tends to maintain itself 'in equilibrium', that is, to react to stimuli in such a way as to return to a state of rest. This is also at the basis of the notion of homeostasis, although this introduces some new ideas as to the mechanisms concerned (feedback, circular processes). Intimately connected with this is the 'automaton model' of the organism, that is, the conception that the organism is essentially a reactive system, set into motion only by external influences (stimulus-response scheme).

These models are unrealistic. That the organism is not a resting but a primarily active system is shown by phenomena as diverse as metabolism, spontaneous movements of lower animals, of foetuses before the establishment of reflexes or after deafferentation (Lorenz, *First Meeting*, Vol. I, p. 108 ff.), the EEG of the unstimulated brain, '*in vacuo*' behaviour, and innumerable others. Liddell gave a nice illustration of the bias imposed by model conceptions (or rather metaphysico-political superstitions) when he told us (Vol. II, p. 142) how for the Pavlov school spontaneous behaviour in the dog was unorthodox, 'against the rules', and politically suspect.

In contrast, according to the open-system model, the organism is an intrinsically active system. Furthermore, the theory of open systems accounts for just those properties of the living organism which were considered 'vitalistic', that is, violating the laws of physics such as the equifinality of development (Driesch's 'first proof of vitalism'), and the apparent contradiction between the trend towards increasing disorder in the inorganic world following the second principle of thermodynamics, and the trend toward increasing order in biological development and evolution (another 'proof of vitalism' according to DuNoüy and others). These vital characteristics are in contrast with the conventional physics of closed systems, but are perfectly legitimate within and, indeed, necessary consequences of, a generalized physics of open systems. Also the problem of biological time, referred to by Grey Walter (Vol. II, p. 34, 66) comes under the theory of Irreversible Thermodynamics and biophysics of open systems.

Both Lorenz and myself (Bertalanffy, 1937, p. 10 ff., 133 ff.; 1952, p. 17 ff., 114 ff.) have for a long time stressed that the organism should be considered as an essentially active system—Lorenz in his theory of instinct and behaviour, connected with von Holst's criticism of classical reflexology; I in the context of general biological theory, eventually leading to the modern expansion of kinetics and thermodynamics, as briefly indicated above.

2. Coming to psychobiological matters, we find the development closely parallel. An important basis of Freudian theory is the 'principle of stability' he adopted from Fechner. The supreme tendency of the organism, biological and mental, supposedly is to get rid of stimuli, and come to rest in a state of 'equilibrium'. It is in the same vein when the concept of homeostasis is applied to any sort of behavioural or mental activity—from mountain climbing to science or composing sonatas (cf. Stagner (1951) and the criticism of the concept of homeostasis by Toch & Hastorf (1955) and Bertalanffy (1951a)).

The above theoretical notions do not seem to account for those aspects which are variously called play-activities, exploring, creativity and the like, going along with 'function pleasure' (Karl Bühler, cf. Mead's remarks, Vol. II, p. 139), which is so characteristic of human behaviour in general, and mental development of the child in particular. As Lorenz has always emphasized (e.g. 1943), these activities have their forerunners in animal behaviour. Just as the physical organism avoids a state of equilibrium, so does the mental organism, an essential aspect of which seems to be not relieving of tensions but rather building up new tensions.

Neo-Freudian theory tries to account for this state of affairs. Thus Alexander (1948) states that 'the basic function of the mental apparatus consists in sustaining the homeostatic equilibrium' but adds that besides the principles of 'stability' and 'inertia' (the latter identical with 'progressive mechanization' as mentioned above) another principle of 'surplus energy' is required. The Montreal experiments, reported at this Conference by Dr. Bindra (Vol. II, p. 75 ff.), dramatically show that the human organism just cannot stand a state of non-stimulation, of complete rest and of 'equilibrium'. The hallucinations occurring in absolute seclusion are a vivid demonstration of the 'autonomous activity' of the psycho-physical organism.

So, physiologically, the organism is an intrinsically active system, tending to a steady state and allowing even for 'anamorphosis', i.e. spontaneous transition toward higher order. Psychologically, this implies what may be loosely called 'creativity'; and in terms of general theory, these are consequences of the organism being not a closed system attaining equilibrium but an open system.

3. Naturally, it remains to be seen in how far the open-system model can be applied to behavioural or psychobiological science. Attempts in this direction have been made by Krech (1950) and Pringle (1951) and in the transactional viewpoint, often quoted nowadays, by Bentley (1950). A 'biologistic' reductionism would be no better than 'physicalism'. Trivially, open systems as treated in physics and biophysics are something quite different from what the psychologist is speaking of. I propose, however, that as a tentative model or analogue, 'open system' with 'autonomous activity' and 'anamorphosis' is a better construct to start with than 'closed system' (which actually is at the basis of Gestalt psychology, behaviourism, cybernetics, and Freudian theory) (cf. Bertalanffy, 1951b, p. 33 ff.), 'primary reactivity' (the stimulus-response scheme), and mental organization conceived as an apparatus to maintain 'equilibrium'.

### *III. Stages or continuous development*

In view of the foregoing, the controversy whether development is 'continuous' or taking place in 'stages' seems to acquire a few new aspects.

1. One relevant notion has already been introduced, namely that of step functions as discussed by Ashby, meaning that the process is not discontinuous but shows more or less rapid transitions toward higher levels or plateaux (cf. the discussion in Bertalanffy, 1956).
2. Obviously there are no all-embracing steps as Gesell (1956) seems to presuppose when speaking of the 'personality of the 10-11-year-old', etc., as distinct entities. So far as somatic development is concerned, a glance at the figure reproduced by Tanner (Vol. I, Fig. 1, p. 37) shows that all organs do not follow the same pattern of development.

There are, however, periods where not all but quite a number of characteristics change. Trivially, puberty is one of them. In this sense it seems legitimate to speak of 'phases' or 'cycles' of somatic and mental growth.

3. As a somewhat less trivial notion, I would like to introduce that of equifinal steps. As has been indicated, equifinality is a characteristic of open systems if and when they tend toward a steady state. Equifinal phases are such as to be reached from different starting points and in different ways, and maintained over a time till a change in conditions—external or internal—causes a new development in the system and brings it on the way toward another equifinal phase. This is related to Piaget's conception of successive equilibria stages or levels, but brings in a few new viewpoints. I am illustrating this by way of a few examples deliberately taken from very different fields.

(a) The early development of an ovum is very different in the various animal classes or orders, being what is technically known as holoblastic, meroblastic, discoidal, or superficial segmentation. This depends upon factors such as the content and distribution of yolk; it varies in different species and is even changeable experimentally. However, an essentially similar two-layered stage, the gastrula, is reached anyway. Similarly, early development in amphioxus, fish, amphibian, reptile, bird, and mammal is very different. Nevertheless, the vertebrate neurula with its characteristic arrangement of primordial organs is strikingly similar in all classes.

(b) Everybody is agreed that species have not arisen by separate acts of creation, as Linnaeus had it, but by natural evolution, presupposing transition from one species to another. However, what we find in nature is separate species, with hardly any intermediates. The nearest explanation is that species are relatively stable systems in genic balance, which therefore show up abundantly in the present fauna and flora and in the palaeontological record, while intermediates are unbalanced, short-lived stages of transition, which for this very reason, usually are 'missing links' (for a more detailed discussion, cf. Bertalanffy, 1952, p. 95 ff.). It is in the same vein that Huxley (Vol. III, p. 200) emphasizes that most species become stabilized at a certain level of organization.

(c) In the history of architecture, we distinguish the Romanesque and Gothic and Baroque styles. There are transitions between Romanesque and Gothic ('Uebergangstil' of the German art historians), and between Gothic and Baroque. However, specimens of these transition styles are rare. Suppose Europe were to be exposed in a new war to even more efficient bombing than took place in the Second World War. Then, for statistical reasons, the few specimens of transition style would be extinguished while a number of Romanesque and Gothic churches would still remain. So the art historian of the future would see a jump from Romanesque to Gothic—exactly the same picture as the palaeontologist sees in the animal and plant world. Romanesque, Gothic, and Baroque would appear to be stages of relative 'balance' which therefore are maintained for quite a while, till new influences (perhaps Arabic in the first instance, Renaissance in the second) upset this 'balance' and lead to a new development and relatively stable state.

Again, early automobiles were a lot of fancy carriages of every imaginable shape, type of propulsion, etc. But when the development became stabilized, that is, a near optimal solution of the technical problem was reached, nothing much happened any more. So present cars of whatever brand are pretty much alike, and the car makers are at pains every year to advertise a 'new' model, that is, one which is a little different in trimming from last year's crop. The parallel to

evolution of new species is obvious. I notice that Piaget has already hinted at 'cultural equifinality' in his questions to Margaret Mead (p. 25).

(d) Something similar seems to apply to the psychobiological development of the child. If I am correctly informed, the age of 10 to 12 represents a stage of internal balance. Then the beginning action of the sex hormones, etc., leads, somatically, to a second acceleration of growth ('adolescent spurt' of Tanner); correlated, electrophysiologically, with the change from theta to alpha rhythm (Grey Walter, Vol. I, Fig. 15, p. 145); psychologically, to a second 'negative' or sulking phase; scholastically, the transition to secondary school (cf. Zazzo, Vol. I, p. 166 ff.). Eventually a new balance is reached in the adult. More detailed are Piaget's (and Freud's) phases of early mental development.

So development seems to take place in a series of equifinal levels, and this seems to take out much of the sharpness of contrast between 'stages' and 'continuous development'. Unnecessary to repeat that these stages are not all-inclusive, and that maturation does not take place simultaneously in all processes, physiological or mental.

## Reply to Comments Concerning the Part Played by Equilibration Processes in the Psychobiological Development of the Child

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This is perhaps not the place to thank my colleagues and to express my appreciation of the very varied replies which they drew up for our last meeting following my attempt at a synthesis. The discussions which took place at that meeting,<sup>1</sup> in addition to indicating clearly my gratitude, showed above all how much attention I have paid to their comments. However, in view of the fact that these discussions have ended in a measure of agreement much greater than seemed possible at the outset, particularly by clearing up certain semantic misunderstandings, I was asked to draw up, after the meeting, a brief reply to the documents prepared beforehand.<sup>2</sup>

The aim of this reply is simply to show that if it is granted (contrary to the impression my report may have given) that the organism is an open and essentially active system then development cannot be explained without having recourse to equilibration processes. In fact, although mental, like physical, life (and even more so) is a perpetual process of construction (and sometimes even of invention), it is by no means incoherent because of this, and what is required is to understand how the mechanism bringing about this continual construction may constitute at the same time a regulating mechanism ensuring coherence.

In the field of the cognitive functions in particular, the problem is to understand how new learning, discovery and creation may not only be reconciled with but take place at the same time as control and verification in such a way that the new remains in harmony with the acquired. This is once more a problem of equilibration. However, although everyone stresses the activity and renewal aspect of development, the equilibration aspect is only too often forgotten. Above all,

<sup>1</sup> Reported in Part II.

<sup>2</sup> It is, of course, understood that I alone am responsible for this reply which must remain without an answer.

it is often not sufficiently realized that these two aspects are inseparable and that the very same agencies which effect the new constructions are also those which simultaneously ensure their regulation.

An example of this is afforded by logical operations, under their double aspect of agencies of indefinite construction and coherent reversibility. Although this example is almost unique, as we shall stress, from the viewpoint of degree of perfection in equilibrated adaptation, it constitutes no more than the final term of a long series of regulations of all kinds, which come into play with the most elementary learning and perception, and whose semi-equilibrated mechanisms of retroaction and anticipation provide the basis for the logical reversibility which characterizes logical operations. Furthermore, this example illustrates well what is doubtless true in general, namely, that analysis of regulation, in other words of equilibration, throws some light on the mechanism of construction itself (in the case of operations, in fact, every new construction, and consequently every invention, is reversible from the outset and therefore can be equilibrated).

1. The result of stabilization and, in particular, compensation processes, can be designated by the term equilibrium<sup>1</sup> or by that suggested by Bertalanffy, 'stable state in an open system'. Whatever the vocabulary employed, however, it must be stressed at the outset that such processes always exist in a living being, which amounts to saying that, for it, equilibrium does not represent an occasional or extrinsic characteristic but an intrinsic one, subsuming a certain number of specific functions. Thus, for a pebble, the fact of being in stable, unstable, or metastable equilibrium in no way affects its other properties: thus, its equilibrium is an occasional or added characteristic and the proof thereof is that in order to define a state of stable equilibrium the physicist calls in a system of 'virtual work' which exists only in his mind and not in the pebble itself. On the other hand, a higher vertebrate which could not stand on its paws would be pathological; here a homeostatic disorder constitutes a disease. From the mental viewpoint, an adult whose thinking remains unstable as regards definitions, inferences or decisions is considered to be abnormal. In each of these latter cases, equilibrium under one form or another constitutes an intrinsic and not an extrinsic characteristic of the fields considered. (Naturally this does not signify that we have here a specific property of life, but only that wherever there is life there is also equilibration.)

2. In the second place, it must be stressed that the equilibration

<sup>1</sup> A translation of the original French word 'équilibre'. It should be noted that in French this word has a broader sense than the words 'equilibrium' or 'balance' in English. See pp. 92, 94.

process which thus constitutes an intrinsic characteristic corresponds, in living beings, to specific needs, tendencies, or functions and not merely to an automatic balance independent of the activities of the subject. Thus, in the case of the higher cognitive functions, there exists a tendency to equilibrium which manifests the need for coherence. In the case of the elementary cognitive functions (perception) the same holds true, although the forms of equilibrium attained are more fleeting and less stable. In other words, the force of the tendency is not entirely determined by its results, and this is why it is better to speak of *equilibration* as a process corresponding to a tendency rather than of equilibrium only.

3. To these needs, tendencies or functions correspond special mechanisms or agencies of equilibration whose activity is complementary to that of all behaviours aiming at the exploration or modification of the environment during the exchanges between it and the organism. Thus, all sensorimotor activity is accompanied by regulation of posture and tonus, etc. In the case of the cognitive functions, one may conceive of the elementary logical operations as constructing new forms or new assemblies within the environment (classifications, seriations, correspondences, etc.); but these activities are necessarily accompanied (necessarily, because this is a condition of their success) by a stabilization of their forms and elements (conservation, etc.). From this viewpoint, it may be said that the inverse and reciprocal operations taking place in this stabilization constitute the equilibrium agencies, it being understood, however, that these mechanisms or agencies are indissolubly linked with those affecting the new constructions.

4. In the sense in which we understand the term, then, equilibrium is therefore essentially bound up with the activities of the organism, not only because equilibration presupposes activities, but also because the stable states or equilibrium forms reached at the end of equilibration processes always represent the play of compensation between activities proper. Stable equilibrium may be defined locally by assuming that if a small perturbation  $\Delta E_p$  is introduced in a state  $E$  by the observer or by nature, the subject reacts by a spontaneous movement of the same order,  $\Delta E_s$ , which returns the system to the state  $E$ , or to a state close to this. It is then said that the reaction  $\Delta E_s$  constitutes an activity.<sup>1</sup>

5. If we prefer the term equilibrium (mobile or dynamic) to that of stable state, it is because the concept of equilibrium implies that of compensation and because the activities of the subject (see 4) are always compensatory at the same time as constructive. This

<sup>1</sup> For more detailed definitions, see the publication on logic and equilibrium which is being prepared by the *Centre international d'Epistémologie génétique*. Geneva.

concept is of general importance, since it doubtless concerns the fundamental mechanisms of assimilation or learning. If these mechanisms are assumed to be on a simple process of association, the problem then remains of understanding why certain associations are unstable (for example, conditioning considered as merely association remains temporary or unstable) whereas others are stable. The problem can be solved only to the extent that a stabilization factor is introduced, in the form of the satisfaction of a need (which is thus a compensation in the sense that filling a gap is a compensation). In other words, in the event of a stable association between  $x$  and  $y$ ,  $y$  is not only associated (externally) with  $x$ , but assimilated to  $x$  in the sense that  $y$  is merged into the  $x$  schema and fills a momentary gap (need) relating to this schema.

These considerations confirm what has been said (under 3) regarding the complementary and indissociable nature of equilibration and of assimilation; the concept of assimilation explains more than does that of association precisely in so far as it includes a stabilization factor.

6. The compensatory activities just discussed (5), which therefore constitute the specific agencies of equilibration (cf. 3), play a considerable part at all levels of behaviour, in the form of retroactive processes necessary for the anticipations involved in construction. In this respect, it may be considered that the agencies of equilibration correspond in general to all regulatory systems in their dual retroactive and anticipatory aspect. However, these concepts recur continually in all theories explaining behaviour, from the 'feed-back' common in the Anglo-Saxon countries to the reafferences and models of action of Soviet psychology. Even in a theory of learning as associationist as Hull's, retroactions play an essential part.

7. However, even if all this is commonplace, it is not often understood that the higher cognitive operations constitute, with their characteristics of combined retroaction and anticipation, structures similar to those of the regulations (and the silence of my colleagues in the study group on this point shows that I continue to be not very comprehensible in this connexion). However, there are two differences, namely, that they attain complete equilibrium and that, thanks to the complete reversibility which characterizes this equilibrium, the operational structures take an algebraic form simpler than the mathematical expression of 'feed-back'.<sup>1</sup>

8. Thus, reversibility for an operation leading from state  $A$  to state  $B$  consists in the presence of an inverse operation leading back from state  $B$  to state  $A$ . Reversibility (in the form of inversion or reci-

<sup>1</sup> These operational structures take simple forms such as groups, groupings, lattices, etc., while a 'feed-back' must be expressed as a complicated integral.

procency) is thus a special case of retroaction: that in which the retroaction brings about a complete return to state *A* and not only to a state *A'*, close to *A*. It may therefore be said that, in the case of operations, operation *BA* is the same as operation *AB*, but reversed (an identity which is indicated by the consideration that when a subject understands an operation he also understands, by this very fact, the possibility of its inverse), whereas, in the case of a regulation, no matter of what kind, the two actions which lead from *A* to *B* and from *B* to *A* or *A'* respectively, are different. Apart from this distinctive characteristic, however, operational reversibility is nothing more than retroaction. It may therefore be said that operations represent a direct prolongation of regulations and it may even be considered that, from the viewpoint of equilibrium, the three great structures which dominate mental life and arise in hierachic order during development are the basic rhythms, the regulations and the operations. These logical structures consequently do not represent an isolated sector of mental life (or a characteristic formed from outside by language, etc.) but the final stage of an edifice all of whose parts are interdependent.

9. However, the value of an equilibration theory is precisely in explaining this completion of the activo-cognitive structures (if they can be so termed). Indeed, it is this progressive equilibrium of the compensation ( $\Delta E_s$  in relation to  $\Delta E_p$ ) which underlies operational reversibility, and not the reverse. If it were necessary to explain equilibrium by reversibility, it would be impossible to understand from whence the latter could arise, whereas one can understand (in outline) how coarse compensations become finer and how, with the aid of symbolic function and representation, these compensations may finally bring about, *in thought*, exact reversibility. To employ a comparison, which is more than a mere image, it might be said that when a physicist describes the equilibrium of a body, he calls into play systems of 'virtual work' which exist in his mind and not in the said body, while in bringing about the equilibrium of his interiorized actions (which are his operations), a living and thinking subject establishes an interplay of compensations between the different components of virtual work, which then play an effective role in his actual thought.<sup>1</sup> This system of virtual work constitutes, in fact, a system of all possible operations for a given structure and it is precisely these possible operations which represent logic.

10. From such a viewpoint, logical structures are the only completely equilibrated structures in the organism (apart from a few similar

<sup>1</sup> We might thus define virtual work without calling on concepts of force, etc., but considering merely  $\Delta E_s$ 's which are imaginable (in the true sense of the word) without being actually carried out.

structures which approach without attaining the same precision, i.e. perceptive constants and certain sensorimotor schemata relative to space and objects). As such, the operational structures constitute a very special case, whose properties cannot be generalized for the whole of mental life, even under its cognitive aspect. But as this special case also represents, at the same time, the final point of a very general process of equilibration and as this process concerns the regulations as a whole (and, beyond them, more basic rhythms), the study of logical structures is very important in order to determine the real significance of equilibrium mechanisms.

11. It should be noted further that, although equilibration thus constitutes a developmental factor to be added to the three classic factors of heredity, environment (external or internal), and social education, it is a factor which cannot be dissociated from them. To be more precise, equilibrium is a form (and equilibration a structuration), but this form has a content and this content can only be hereditary or acquired by physical or social learning. However, as none of these three factors acts alone, it would be useless to try to isolate the equilibration factor; it intervenes in every hereditary or acquired process, and intervenes in their interactions. It is in this sense that it is the most general of the four, but this in no way signifies that it is superimposed on the other three by an additive process.

12. In particular, the equilibrium factor is dominant in exchanges between the organism and the environment. These exchanges correspond to what is generally termed 'adaptation' (and Lorenz suggested that I replace the term 'equilibrium' by 'adaptive interaction'). However, all adaptation, both mental and physical, includes two poles: one corresponding to the assimilation of energy or matter from the environment by the structure of the organism (or mental assimilation of data perceived in the environment to the schemata of action followed by the subject); the other corresponding to the accommodation<sup>1</sup> of structures of schemata of the organism or subject to environmental situations or data. Adaptation is then nothing more than an equilibrium between this assimilation and organic or mental accommodation. This is why the most elementary exchanges between subject and object are already determined by the equilibrium factor.

### *Conclusion*

This last remark (12) enables us to conclude by putting the equilibrium factor in its true perspective, which is a biological and not a

<sup>1</sup> We use this term in the sense of 'accommodates' or phenotypes, i.e. variations undergone by the organism in relation to the environment.

logical one, although the special equilibrium of logical structures is one of the finest achievements of living morphogenesis.

We shall therefore conclude by saying that life, like thought (or thought, like life) is essentially active because it constructs forms. From this viewpoint, thought forms are a prolongation of living morphogenesis through the intermediary of nervous co-ordination, sensorimotor schemata of action, etc., without forgetting social structures, since the operation of reason is always dependent on co-operation. However these forms or structures, whether biological or mental, must constantly comply with the double requirement of assimilation of objects or external data to them and, in return, of accommodation to these objects or data. Without assimilation, the organism or subject would be like soft wax, as in the reproach levelled against empiricism, ceaselessly modified by chance encounters or changes in the environment. Without accommodation, the organism or the subject would be withdrawn within itself and beyond the reach of any external action. This equilibrium between assimilation and accommodation can only be limited and relatively unstable on the organic level, since the effects of one are attained at the expense of the other: equilibrium is only a compromise at the level of organic morphogenesis or variation of the species. With nervous organization and mental life, on the contrary, a twofold power of retroaction and anticipation, of reconstitution of the past and the foreseeing of the future, considerably enlarges the field of this equilibrium and replaces fleeting compromises by actual syntheses. Schemata of action already constitute such syntheses, with their power of general assimilation and multiple accommodation. Nevertheless equilibrium is only attained, from the operational and cognitive viewpoint, with logico-mathematical structures capable of assimilating the whole universe to thought, without being ever broken or even shaken by the innumerable accommodations called for by experience. We have studied the background of this cognitive equilibration in the modest sector represented by child development: but, even within this limited field, it is remarkably instructive and becomes much more so once properly situated in its general perspective.

PART II  
DISCUSSION

## INTRODUCTORY DISCUSSION

DR. CANDAU (Director-General, World Health Organization):

Ladies and Gentlemen; I am very happy to welcome you here at Geneva today and to open the fourth meeting of the Study Group on the Psychobiological Development of the Child, which is to be the last of the present series. As I understand it, the Group intends, during the coming week, to try to synthesize the points of view of the disciplines which it represents and to reach some general agreement as to the interaction of factors involved in the child's development, the way characteristic stages follow one another, and the actual mechanisms involved when one stage succeeds another. It has been agreed that searching for a common language to facilitate understanding between representatives of different specialities is of fundamental importance.

Considerable preparatory work has been undertaken for this meeting. Professor Piaget, who was requested to make the initial presentation, has circulated to the Study Group members a stimulating paper on the general problems of the psychobiological development of the child. This has evoked much comment from the members, and a series of preparatory papers give their points of view and replies to specific questions addressed to them by Professor Piaget (see Part I). I am happy that the World Health Organization has been able to contribute to the preparation of this meeting, in particular by aiding in the making of a film by Professor Piaget and Mlle Inhelder on some aspects of the child's intellectual development. I look forward to the further discussion with very great interest.

FREMONT-SMITH (Chairman, Study Group):

We have much reason to be grateful to Dr. Candau, and first I want to express our thanks to him, and to Dr. Peterson and his staff. We are particularly grateful for the fact that we have been clearly designated as a Study Group which is distinct from an Expert Committee, and given the job not of passing resolutions but of interacting, learning, growing, doing something which we are studying in this Study Group on the Psychobiological Development of the Child—developing ourselves.

Professor von Bertalanffy, as our only new member on this occasion, would you be kind enough very informally to introduce yourself by telling us in three or four minutes those highpoints in your career which you think we would like to hear about? (See Vols. I, II and III for similar introductions by other members.)

When I read the proceedings of the previous meeting I noticed that in introducing themselves the members of the Group emphasized nurture and somewhat underplayed nature, even though in many of you—for example Julian Huxley and Konrad Lorenz—the hereditary traits would be quite obvious. So, for a change, let me reverse this procedure. As my name reveals, I am a Hungarian of a sort, meaning that my stock is Hungarian even though I do not speak a word of this language. The first Bertalanffy of some shadowy literary merit appears to have been a Jesuit, Bertalanffy Pál, who in the early 1700s wrote a pious book of sermons. He, having been a Catholic priest, obviously cannot be in my direct ancestral line. To what extent traits of the Jesuitic mind are discoverable in my way of thinking is for you to decide. Anyway, this book seems to be one of the first printed in the Hungarian language and quite a bibliophilic item. Only two copies have come to my attention—one was once in my possession and was burnt with my other belongings during the war, and the other one is in the British Museum.

One of my grandfathers also was quite a character. He was a trained lawyer, but eloped with a wandering theatre troupe—with a road-show I think you would say—and ended up as the director of the Stadt Theatres in Graz and Vienna. I read in a book that the Graz theatre was never at a lower artistic level than under my revered grandfather's direction, but also was never better off financially. Unfortunately I did not inherit his businesslike mind.

As far as I myself am concerned, my work has been in the field of theoretical and quantitative biology, experimental medicine, and cellular and comparative physiology. In the present connexion, however, my philosophical background will perhaps be more interesting. It is a rather chequered one. In my youth I was strongly attracted by German mysticism. Indeed, it is one of my small triumphs that back in 1928 I published a selection of the works of Nicholas of Cusa—that extraordinary figure of the Quattrocento who, being a Roman Cardinal, was at the same time the last of the line of great German mystics and the first of the line of modern science and enlightenment which later led to Copernicus, Galileo and Newton, as well as to Giordano Bruno and Leibniz. This was some years before Nicholas of Cusa was brought to the more general attention by the Heidelberg Academy's great edition of his works. On the other hand I was a student and pupil of Moriz Schlick, the founder of the Vienna circle of logical positivism. I was also on very good terms with the blind philosopher Hans Vaihinger, the author of the *Philosophy of As If*. Such influences at least lead a person to be broadminded, and what today I would call a 'perspectivistic' viewpoint is, in modern terms, the dictum of Nicholas of Cusa 'ex

omnibus partibus relucet totum', meaning that any part or any viewpoint in some limited way reflects the universe.

If I am asked what I would consider as my moderate achievements, I would perhaps point to the following. First, I have been advocating that viewpoint in biology known as the organismic conception. The concept that the organism must be considered and investigated as a whole is a triviality nowadays, but it used not to be so, and was indeed under heavy fire when I tried to advance it during the 1920s. This viewpoint was partly the background of a good part of my laboratory work, which was concerned with metabolism, growth, and related problems in their quantitative aspects. Furthermore the concept of the organism as an open system and a steady state led me into physical chemistry, kinetics and thermodynamics, and here I was confronted with a rather amazing fact. Obviously the organism is a system showing continuous exchange, import and export of matter and is thus an open system—but there was no theory in conventional physics to account for such systems and their laws. Eventually I was led to the concept of the general system theory as a sort of generalized superstructure of science, applicable particularly to fields outside physics.

After many years at the University of Vienna, the course of my life has led me, with the exodus of Austrian scientists after the war, first to Canada where I directed a department of research in medical biology at the University of Ottawa, and then to the west coast of America. After a year at the Centre for Advanced Study in the Behavioral Sciences in Stanford, I recently came to Los Angeles. My laboratory is particularly engaged in cancer work, and at present we are studying the application of fluorescence microscopy to cancer diagnosis. However in recent years my interests have increasingly concentrated on behavioural science and the basic theoretical questions of psychiatry.

Now it remains to me to thank you for your kind invitation, and I certainly do not need to tell you how much I am looking forward to this conference, hoping to contribute whatever little I can to the fundamental problems this Group is investigating.

FREMONT-SMITH:

Thank you very much, Dr. von Bertalanffy.

PIAGET:

This essay of mine (Part I) which was sent to you was a kind of experiment. When I tried to carry out the task which you gave me the honour of undertaking, that is, of attempting a synthesis as a starting point for our discussions, I found myself in a very awkward situation. A synthesis is either a common doctrine—which is

obviously impossible in a group such as ours with such multiple and diverse points of view—or it is a common hierarchy of the problems, a common search for general ideas or general models. Now to write a paper simply on the problems and ideas that can be used would not be very exciting. I therefore chose to make a sort of compromise; I tried to draw up a paper which would stress particularly the problems of a common language, of the factors affecting development, and of developmental stages and the mechanisms of transition from one stage to another. Apart from this, I included in my paper some personal opinions which I have not developed fully, but which I put in, to be quite frank, as a challenge to provoke contradiction and discussion. I added a few questions for the same purpose.

The experiment I wanted to make concerned the two following questions: (1) would there be real reactions, by which I mean reactions not merely for the sake of courtesy, but constituting a real discussion of the problems posed, and (2) would these reactions be relatively convergent or, on the contrary, would they disperse in all directions? If the responses were real and sufficient convergence were achieved, we could consider the experiment successful and consequently speak of the possibility of making a synthesis; otherwise it would have meant failure, and we should have had to look for something else.

I hope it does not surprise you if I conclude that there was a much greater convergence in your replies than one might have expected. It is a convergence which is partly implicit and our job here is, of course, to get it to stand out more clearly. It is a convergence also achieved frequently at my expense, that is, in contradiction to what I said. That is of no importance, that was just the point of the experiment. Moreover, I think that as regards this last point it is more a question of appearance than reality, and the lack of agreement which may be found between your responses and my paper is not very fundamental. I think I can agree very sincerely with almost all the objections which have been made to my paper, except for one point which is of major importance to me, and that is the role of logic in cognitive functions.

On reading your replies I got a very encouraging impression of freshness, of much more that was new than each of you would probably imagine from your own reply. Authors frequently make themselves best understood in connexion with a discussion of general problems, and I have to admit that on reading your papers I understood a number of points concerning your thoughts which I had previously missed. I am forced to confess that up to then I had misunderstood certain points in the thinking of Margaret Mead, for example, and of Bowlby, of Lorenz, and of several others, and that their replies seemed to me not only highly instructive and illuminating, but also very constructive as regards the synthesis

which we are seeking to establish during this last meeting of the group.

I should like to give a few examples of these new points contained in your papers and about which it seems to me almost unanimous agreement is possible.

As the first example I will take the problem of stages as dealt with by Bowlby in his reply. I had already understood from previous discussions between Bowlby and myself that we fundamentally agreed on the problem of relations between an individual's past experience and his present organization. I had understood that Bowlby does not consider, as did Freud in certain passages, that we are fixed in the past and that there is necessarily regression into the unconscious during the utilization of this past, but rather that the past is continually reorganized according to present needs and present structures. But though I had already understood this point, I did not know how to reconcile the Freudian stages with the stages of cognitive development in the child. Now, Bowlby's reply contains an idea which seems to me very clear and fertile. He says that an affective reaction in the child—for example, the relations between the child and his mother—is the product of a group of reactions which are at first isolated or unco-ordinated between themselves, such as suckling or smiling or imitating, but later becomes more and more closely co-ordinated until they finally constitute a whole. Thus Bowlby suggests—and this is the idea which seems to me fundamental—that the stages which are important in the field which interests him are not stages of the same type as the Freudian ones which stress chiefly the dominant aspect at a certain period and which refer essentially to the content of behaviour patterns and not to their form. They are instead stages which depend chiefly, if I have understood Bowlby properly, on the progressions in this development from isolated elements into a co-ordinated whole. In these processes of affective co-ordination, then, we can find stages which might correspond to, or would at least compare more closely with, the stages of cognitive development.

I will take another example, that furnished by Lorenz's reply. I am forced to confess after reading it that up to now I had not understood Lorenz at all well. I had realized this and attempted to do something about it—in particular I suggested that one of my students at the Sorbonne should write a thesis on the ideas of Lorenz and Tinbergen; the thesis was excellent, but did not entirely clarify my thinking. But Lorenz's paper in reply to mine contains a series of statements which seem to me of great importance, and are hard to find in his other writings. For example, I had always thought that his idea of spontaneous activity of an organism was tinged with apriorism or almost with vitalism; but in his paper Lorenz makes

an absolutely clear statement against apriorism and preformism in favour of a constant interaction between the spontaneous activities of the organism and environmental influences, so that I am entirely in agreement with this whole section of his reply. In my mind, 'spontaneous' had seemed to contradict this constant interaction; if this is not so, the synthesis of our views becomes easier. Instinctive activities have to be considered as a particular case of morphogenetic activities in general, and I think that cognitive activities are also comprised in morphogenetic activities. It follows that all the work of Lorenz's school on this morphogenesis from the point of view of 'spontaneous' activity could be of direct interest to my research on the development of cognitive structures.

Now I am coming to the main point of my presentation which was discussed by everybody: the idea of equilibrium as a factor in development. On this point there is a very remarkable convergence in your written replies. All the replies made reservations and alterations along the same lines.

- (1) Bertalanffy's paper proposes substituting for the word 'equilibrium' the idea of a stable state in an open system.
- (2) Lorenz would agree with what I wrote if I replaced 'equilibrium' by 'adaptive interaction'. I think that the idea of adaptation presupposes precisely an equilibrium; in adaptation there is always an assimilation of external factors, whether energetic or chemical, etc., from the environment by the organism and, on the other hand, an accommodation of the organism to the situation in which it finds itself; the adaptation then is already an equilibrium between assimilation and accommodation, and that is why I prefer personally the term 'equilibrium'. But the idea of adaptive interaction is absolutely fundamental.
- (3) Margaret Mead says she agrees with the language of equilibrium but she wonders whether the model of gains and losses is the best model. I shall come back to this, if you permit, when we speak of cognitive equilibrium; for the moment I would simply mention that when speaking of gains and losses I am of course speaking of gains and losses of information and I remark that the word used by Margaret Mead to ask whether this model is the best is the word 'rewarding'. Well, it is precisely in the sense of 'rewarding' that I am speaking of gains and losses; that is to say that there are ideas which are rich in information, and there are ideas which lead to losses.
- (4) Grey Walter makes two remarks in his paper which seem to me fundamental to our discussion. The first remark is that the best dynamic equilibrium always corresponds to the maximum activity. The second remark is that equilibrium and in particular equilibration is a particular case of a stochastic process and that the idea of equilibrium must be interpreted in a probabilistic sense.

(5) Finally, everyone made the remark that the organism never is in equilibrium.

Here then are the different points on which I received replies. I think personally I am about a hundred per cent in agreement with these different remarks. I think that the divergence depends mainly on terminology, on vocabulary. There is only one point where I do not agree with the replies given on this idea of equilibrium and I will begin with this point so as to clarify what I wanted to say.

Here is the question. Naturally I am ready to admit that the organism is never in equilibrium; equilibration processes always occur but we never find complete equilibrium is attained. There is, however, a special field where one can speak of equilibrium (which is why I am preoccupied by this idea) and it is precisely the field of logical structures. Take logical or mathematical structures such as the system of whole numbers. When a child of seven or eight years has managed to construct a series of whole numbers, when he has understood the series 1, 2, 3, 4, etc., then this structure will remain in equilibrium (if you prefer another word I do not mind) until death unless the individual goes mad. Now this does not mean that there is any state of rest; the individual will all the time make use of these ideas in action on objects or in exchange with other individuals. This system will also be integrated into other systems; that is to say that after having learnt whole numbers the individual will discover fractions, irrational numbers, complex numbers, transfinite numbers and so on. But whatever the new systems into which the system of whole numbers is integrated, this number system will not be changed any further. It remains in an equilibrium which is characterized by mobility and activity and which is founded on operations constituting actions of adding a unit or taking one away.

This equilibrium is characterized above all by the idea of reversibility, a reversibility which I will define as being the possibility of making an operation correspond to one and only one reverse operation which cancels it out. Now, the point that I did not manage to make understood, it seems to me, is that this reversibility of mental operations, which characterizes the equilibrium of a cognitive structure, constitutes the limiting case or final outcome of a process which is of much wider occurrence than logical structures. This process is found in all fields which deal with retroactive mechanisms, feedback mechanisms, anticipatory mechanisms, etc. I will take an example of behaviour showing these retroactions or anticipations which ensure a certain equilibrium: it is the classical example which has been quoted everywhere as being a suitable demonstration of feedback in the ordinary behaviour of individuals, the example of a driver on an icy road who tends to slide to the left. He makes a correction to the right, tends to slide back again to the right, corrects to the left

and so on. At first he makes his corrections with big movements causing wide oscillations but after a time he manages to make corrections in advance, that is to say, by progressive anticipation before there has been any deviation. There you have an equilibrium which derives from probabilistic considerations: every movement made by an individual, each of his successive actions, comprises a probability determined by the resultant of the preceding behaviour according to a series of sequential controls easily describable from the probabilistic point of view. With the trial-and-error retroactions and anticipations of ordinary behaviour you never have complete compensation; thus though you certainly find equilibration processes you never get to perfect equilibrium. With intellectual co-ordinations, on the other hand, you have a system of operations which finally does reach equilibrium. These operations, since they are actions to start with (the operation of addition is the action of putting together; the operation of subtraction is the action of taking a part, etc.) remain actions like the others, actions which first gave rise to tentative trials and obey the same laws as the actions of the driver on the icy road. However, they show this notable peculiarity that, in the course of sufficiently general actions, such precisely as logical operations, the compensation obtained becomes finally a complete compensation.

But this is only a particular case in the wider problem of equilibration processes in general. As far as this general problem is concerned I think, in common with all the authors of the replies, that equilibrium in the sense in which I use the term is not a state of rest but essentially an active state and that the best equilibrium will always correspond, as Grey Walter said, to a maximum of activity. I think, secondly, that equilibrium is never achieved except in the case of certain special structures like the logico-mathematical ones. Finally, I think, as Grey Walter suggests—that all these equilibrium mechanisms have to be interpreted in a probabilistic, stochastic fashion.

I wanted to present these few preliminary remarks in order to show that there is a much greater convergence than one might have imagined between my paper and the replies.

In consequence, we can justly consider the making of a synthesis at this last session, a synthesis which will not be one of doctrine of course, but which will consist of an arrangement in order of possible questions and explanatory models, so as to delimit the field of interdisciplinary research which will be most usefully followed in our subsequent work.

BERTALANAFFY:

'Equilibrium' is a term that presents considerable linguistic difficulties. The terms are well defined in English, but the French expres-

sion, *état stationnaire*, is equivocal, and in German I had to introduce a new term, *Fliessgleichgewicht*, which is now generally accepted. It might be of some use if I give a precise definition of the terms relating to 'equilibrium'. These definitions are in terms of physics, but are applicable to biological phenomena.

(1) A *stationary state* (*état stationnaire*; *stationärer Zustand*) means a state of a system where the rate of macroscopic changes becomes zero; that is the system does not show macroscopic changes in time. This is irrespective of the kind of system.

(2) An *equilibrium* (*équilibre*; *Gleichgewicht*) is a time-independent state of a system where all macroscopic magnitudes remain constant and all macroscopic processes stop. A state of equilibrium must be attained in a closed system according to the second law of thermodynamics and is defined by a maximum of entropy.

(3) A *steady state* (no accepted term in French; *Fliessgleichgewicht*) is also defined as a time-independent state where the system remains unchanged macroscopically, although macroscopic processes of import and export of matter are going on—in contrast to the equilibrium state of a closed system. There is no simple thermodynamic definition for a steady state and it is not defined by maximum entropy; it can be defined in kinetic terms.

These are the meanings so far as physical chemistry is concerned. I should mention that, for example, the equilibrium which is spoken of in stochastic models or in game theory is again something different.

#### PIAGET:

If I understood Bertalanffy properly, he has defined 'steady state' exactly in the sense in which I speak of equilibrium in my parlance. In our Centre International d'Epistémologie Génétique here in Geneva, we have been searching for a year, with the assistance of a physicist, Mandelbrot, for an idea which might be applied to psychological behaviour which would not imply physical apriorism; something analogous to the idea of forces, or minimum potential energy, which themselves have no meaning in psychology. I would propose then the following definition (Apostel, Mandelbrot & Piaget, 1957).

The most general definition on which we can agree starts from the state  $E$  which is relatively stable, that is, does not change macroscopically. Then there are introduced, either by nature or by the observer, perturbations in the experiment which modify the set-up. Thus you have a perturbation  $\Delta E_p$ . Then a 'spontaneous' movement of the organism or of the subject, which I would call  $\Delta E_s$ , may respond to this perturbation. Now if  $\Delta E_s$  is equivalent to  $\Delta E_p$ , that is to say, brings it back to the initial state, I will speak of

equilibrium. I am avoiding the 'equilibrium of forces' which would have no meaning in psychology and I am avoiding using the term 'minimum potential energy'. My general definition corresponds to Bertalanffy's 'steady state' rather than to the word 'equilibrium', but in French I prefer the term 'équilibre'—because it introduces the idea of compensation. This idea of compensation is of central importance because it is by studying the compensations that one can establish degrees of equilibrium. Thus it is ascertained that imperfect compensations can exist, for example in the trial-and-error and in the retroactions and anticipations which I was speaking about a little while ago. But you can have a strict compensation between  $\Delta E_s$  and  $\Delta E_p$ , that is to say that here the equilibrium will take the form of a group, that is the transformation 1 will be corrected by the transformation  $1^{-1}$ . Thus you have reversibility, or strict compensation as a limiting state towards which the compensations brought into proximity will tend.

You see that this vocabulary is exceedingly simple and makes no physical presuppositions. However, there remains the whole immense problem of the relations between equilibrium and chance. Is this equilibrium, this equilibration, the result of a series of probabilities developing in an analogous way to developing entropy or, on the contrary, is it a question of antichance or of an organizational process resisting disorder?

BERTALANFFY:

I have two remarks to make to Professor Piaget's definition. One is that the definition Professor Piaget gave now is a bit more general than the considerations in his paper, where he refers to stochastic processes, game theory and so forth. And secondly that all these concepts are models we apply, and we can take what model we please for our particular purpose. For certain purposes, for example, if you consider physiological processes and the like, the model of the open system will prove very useful and important. For other considerations, you will use a model from information theory or game theory. It depends on the problem; your only obligation is when you have chosen a model, to stick to it and apply it consistently.

FREMONT-SMITH:

Would you agree that you may also need to use several models simultaneously sometimes in relation to the same problem, to give flexibility of approach?

BERTALANFFY:

I absolutely agree and could even give illustrations of this from physics. In thermodynamics, for example, you can describe things in

classical terms, using concepts like total and free energy, entropy, and so forth, and you can also describe the same processes by statistical mechanics which speaks in terms of probabilities and uses quite a different language. Furthermore, you can translate one into the other. Something similar is true for wave mechanics and quantum statistics according to Schrödinger and Heisenberg respectively. Here also are two descriptions using different mathematical structures, but one model and set of laws can, so to speak, be translated into the other.

## SECOND DISCUSSION

### Equilibration and the Development of Logical Structures

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PIAGET:

Today I hope to reply to the different questions put to me and to speak about the factors affecting development and about equilibrium. I will begin with factors affecting development. I said that we must add to the three classical factors of heredity, physical environment and social influence a fourth factor, equilibration, that is the equilibration of the three factors between themselves and equilibration within each one.

The three classical factors are indissociable and non-additive. For example, the influence of the environment is not simply added to that of heredity, because the organism chooses one or other environmental influence according to its hereditary structure, so that there is from the beginning an interconnexion between hereditary influences and influences from the physical environment. Now if the co-ordination of these influences is not simply the product of a chance mixing then necessarily the problem of equilibration arises. This is why I think a fourth factor is necessary, a factor which plays a part in the actual connexion of the three other factors. Zazzo, in his reply to my paper, says that this factor is no more than a system of relationships, whereas the other three factors seem 'real' to him. Personally I do not think that there is a difference in kind between the equilibrium factor and the others, because as soon as one analyses closely a hereditary mechanism, or an exchange between organism and environment, or more particularly a social mechanism, one finds they are only increasingly complex systems of relationships. One could perhaps say that the equilibrium factor is unlike the others in that it does not derive from simple causality; but it is necessarily inherent in a statistical form of causality. To speak of heredity, of environment or of society as causes in the sense of classic causality is certainly no more than a first approximation, and as soon as one goes beyond this one comes to probabilistic systems and statistical causality.

Now I want to repeat that the problem of logical structures is a special one because this is the only field where one attains complete equilibrium. I think that in the affective field one would also find the equivalent of what logic is in the cognitive field; it would be structurations of social concepts in the form of scales of moral values; however this does not concern me and I will limit myself to what I have experience of, that is to the facts of logical structures. I will take first of all as an example the ideas of conservation. We observe that these ideas of conservation cannot be explained by the three classical factors alone. For the child to reach the idea of conservation (normally at about 7 or 8 years), of course maturation must have occurred: a prerequisite is that the nervous system should have reached a certain level of functioning which derives from a hereditary element. But this hereditary element is not sufficient, that is to say these ideas of conservation are not innate; one observes their gradual construction under the influence of experience. Also they vary very much from one child to another, and particularly from one social environment to another.

Secondly, this is not a mechanism which can be explained simply by physical experience or by the experience of objects. Of course experience plays a part; children manipulate objects and learn by observing the results. But this factor of experience is not by itself sufficient for the following reasons. The first is that we have never seen a child spontaneously verifying any conservation by systematic experiment. Either the child does not believe in conservation or else suddenly he believes in it and when he believes in it there is a totally different approach, and an expression of full understanding, of full comprehension of a new intellectual instrument; he says that it is obvious that there is conservation. We have never seen a child not believe before, then make an experiment, and then believe afterwards. Sometimes we encourage him to make experiments, for example on the conservation of volume with the water level, or the conservation of weight, giving him a balance, but the child does not try of his own accord. In the field of physical conservation, it is most remarkable that the first of the ideas of conservation to appear is not that of weight or volume but of substance. What is substance without weight and volume? It is nothing at all, it cannot be shown by experiment, it is a kind of empty form which begins by being the first idea of conservation; it is not a product of experience. Finally there is feeling of necessity; conservation appears as obvious as soon as it is understood. The feeling of necessity does not come from experience; it is the accompaniment of the understanding of the logical relationship.

Thirdly, of course, there are the social factors in this establishment of conservation, but these factors are not by themselves enough either.

One can give all kinds of proofs of that: for example, the most primitive form of conservation, the permanent object on the sensorimotor level, appears before language. Then comes the formal level when the child reaches structures of groups with four transformations (inverse, reciprocal, correlative and identical); these structures go beyond language, they cannot be expressed by language and yet they influence thought in a very clear manner. Also there is a temporal sequence in which these notions appear and this cannot be explained by language or social factors. In manipulations of the ball of clay, for instance, the idea of conservation of substance is seen on the average at about 7-8 years; weight at about 9-10 years; and volume at about 11-12 years. But the child arrives at these three stages exactly by the same reasoning using the same words. If this were the effect of a verbal structure why should he not apply it immediately to everything? And then finally, the fundamental experiment, there are the deaf-mutes. Oléron (1951) has shown that operatory structures develop quite normally in deaf-mutes. Of course in deaf-mutes there is some symbolic function, such as language by gesture, etc., but there is not the normal social transmission by language. Consequently I think that in this field none of the three factors is sufficient alone; one must have recourse to the factor of equilibration.

During development we observe that there are stages where the child's ideas of conservation, etc., are not stable, but alter. Later the child arrives at a stability of these ideas which normally would last throughout the whole of his life. Now the problem is how to explain this progressive stabilization. First of all I will try to translate equilibrium in terms of activities and strategies. There is one preliminary circumstance we must note in order to understand the strategies which are to be mentioned. It is that in all arrangements where the problem of conservation appears we have to deal with two kinds of qualities or variables which from the observer's point of view vary inversely one to the other. Objectively therefore we are from the beginning faced with a group structure. Take for example the ball of clay which you change into a sausage: it gets longer: that is the first variable, but on the other hand it gets thinner: this is the second variable, which gets modified inversely as the first. Take the same ball and cut it up into pieces; these pieces can be few in number—the first variable—and they can be large or small—the second variable—and if the number is large, the pieces are small; if the number is small, the pieces are large, and we have again two inverse variations. Take the correspondence between elements which you space out: you have the length of the series which increases, but you have the density which decreases. Take the two horizontal bars which are then made to overlap: one projects beyond the other at one side but it recedes compared with the

other on the other side; thus we again have two complementary variables.

I shall call the first variable characteristic *A* and the second characteristic *B*; *A* is not necessarily positive and *B* is not necessarily negative; they simply vary inversely one with the other. Having said this we observe through studying the reactions of these arrangements certain common mechanisms which I shall call strategies. We can distinguish four strategies: (Apostel *et al.*, 1957). (1) *First strategy*: the child deals with only one of the two characteristics. This is what I shall call a focusing of the attention on one, *A*, of the two characteristics. The child ignores the other, *B*. The child says, for example, that the clay ball has got longer and that consequently there is more clay; he does not trouble about its thinness. Or else on the contrary he says that it has got thinner, therefore there is less clay; but he does not trouble about the length. (2) At a given moment appears the *second strategy*: the child deals with characteristic *B* which he suddenly discovers; but he then forgets characteristic *A* and he will go on reasoning according to characteristic *B*, ignoring what went before. (3) *Third strategy*: oscillation between *A* and *B*, with the beginning of a link between the two, but first one and then the other is uppermost. At times there is a direct oscillation from one to the other and at times the child reconciles them for small changes but is not able to do so for big changes. We have here an intermediate strategy, wherein one can distinguish substrategies or particular tactics. (4) Finally you have the *fourth strategy* which consists in understanding that the two characteristics are inversely related to each other as if they were balancing each other. At this moment the idea of conservation appears.

We note therefore a progression; *A*, *B*, oscillation between *A* and *B*, and finally linking up of the two. Let us now try to translate these strategies in terms of a theory of games. Let us attempt to speak of gains and losses of information. We note in fact that there are strategies which are more or less costly and correspondingly produce a greater or smaller yield. The primitive strategy of focusing on one single characteristic is the simplest and least costly. It costs no effort of information—there is simply a focusing of attention on one point with neglect of all the rest and the reasoning starts just from these data. At the same time it is the strategy which gives the least yield; no security, no possibility of prediction; at each modification of the arrangement, the child hesitates again; there is no stable generalization, there is no instrument which permits deduction. At the other extreme we note that strategy 4 is the most costly—it presupposes progressive synthesis; the very fact that there are intermediate stages shows how much has to be elaborated in order to achieve the idea of inverse relationships. But though the most costly, at the same time

it is the one which represents the greatest yield from the point of view of security, and from the point of view of the possibility of prediction, because the child discovers a general law which is going to be applied to all transformations in a particular field.

Since we are faced with a sequential process in which each element is determined by the preceding one, I propose to explain the choice of these successive strategies using a probabilistic language. I am using probability in the statistical sense of the probability of a certain event occurring out of a universe of possible events. The problem then is to know what are the possible events and to make an exhaustive list of them. In this case I will propose a simple solution. We shall call possible cases those cases which can be realized and we shall choose as a criterion of what can be realized that which actually is realized at the end of the process. In other words, the possible cases will be all those which are attained at the last stage. We can then construct an exceedingly simple group of events starting from this criterion. We have the events *A or B*, the events *A and B*, and furthermore we have the transformations leading to these states. The child can deal either simply with these configurations—he looks at the changed ball, he does not trouble about the transformation itself—or else he can deal also with the transformation. What has happened between the initial state and the final state? The ball has been drawn out and this transformation then controls both *A* and *B*.

Having admitted this group, we have first to explain why strategy 1 appears first. The hypothesis is that the choice of a single characteristic out of the two is the most probable solution. Why? Because the facts show us that the child can reason on one out of two characteristics without troubling about the other. From the point of view of the subject the two characteristics appear to be independent, although from the point of view of the observer they constitute a group structure. However we are reasoning from the point of view of the subject and not at all from the point of view of the observer. Therefore, if the probability that *A* will be focused upon is 0.5 and that *B* will be focused upon is 0.5, for them both to be focused upon at the same time would be 0.25, that is to say the product of the two. It is obvious then that strategy 1 is the simplest and the most probable at the beginning in the absence of all information on the structure of the objective relationships which come into play.

Secondly, the child is going to generalize—he starts for example from the length in the case of the ball which you are continually drawing out. He continues to reason about length, using his first strategy in all situations as long as it works. However, at a given moment it no longer works. The problem arises then of the transition from strategy 1 to strategy 2, a problem which does not comprise any modification in the body of events, but which must account for

a modification in the probabilities attached to these strategies. I shall call probability *A*,  $p_1$  and probability *B*,  $p_2$ . At first  $p_1$  is greater than  $p_2$ , but at a given moment we have a reversal which characterizes strategy 2. This reversal may be due to external or internal causes. One of the external causes is the effect of perceptive contrast. One cannot reason indefinitely on the length. When the clay ball is changed into a sausage which lengthens into a string, the child ceases to say that there is more clay than at the beginning. At a given moment the other characteristic, the thinness, forces itself upon his attention. Secondly, there are internal factors which must derive from questions of subjective security or insecurity. In particular there is a lack of satisfaction about always using the same solution when it is not accompanied by certainty. Therefore both for reasons of subjective lack of satisfaction and objective contrast, character *B* finally takes precedence. And when *B* takes precedence, the same reasoning begins over again, but in the reverse sense.

However, this cannot go on indefinitely because even at the beginning there was a certain focusing upon *A*. That is to say that at a given moment the child who is reasoning this time on the thinness and forgetting the length is going to remember the length. An oscillation is then produced, which is the most probable strategy when the subject has gone from *A* to *B* and then recalls *A*. We observe then the whole classical process: oscillation first with retroaction, but later becoming anticipatory. From the moment the child oscillates between the two characteristics *A* and *B*, he begins to foresee that one of the characteristics is not modified without the other. In other words, the probability that *A and B* will take precedence over *A* alone or *B* alone occurs as soon as a retroactive process is produced. It becomes increasingly strong when the oscillations are at the same time retroactive and anticipatory. When the subject reaches this stage, the consideration of transformations is introduced. Up to this point the child has reasoned only on *configurations*—which is much simpler: the configuration is the state of the moment which can be concentrated upon without the other state being dealt with. Thinking about a *transformation* is on the contrary thinking about several states at once, about the point of departure and the point of arrival, with intervals between the two which are also states. The same reasoning that I indicated just now to show that *A and B* are less probable than *A or B* explains why the child begins with configurations. But as soon as these processes of retroactive and anticipatory oscillations occur, the consideration of transformations becomes probable. It becomes so because the subject possesses the two extremes of the transformation and all that is left is to introduce the transformation itself.

In short, the pattern of my explanation would be the following:

you are dealing with a series of sequential controls, such that each strategy is made probable by the preceding one. Strategy 4 is not the most probable at the beginning: it is the least probable then, but each strategy becomes the most probable following the results of the preceding one.

A few more words about operatory structures themselves—only a few words because the process is the same. Within the framework of an operatory structure like seriation (the placing in order of varying-sized sticks, for example), characteristic *A* would be the relationship 'greater than' (or 'smaller than') and characteristic *B* would be the inverse relationship. The child reasons first of all on only one of these relationships. To reach the correct seriation he must co-ordinate the two relationships. The operatory method in seriation is acquired at the precise moment when the child reasons at the same time about *A* and about *B*, that is to say when he looks for an element which will be at the same time larger than all the preceding ones and smaller than all those which remain to be classified. Thus strategy 4 here corresponds exactly to the definition above: that is to knowing how to manipulate simultaneously both characteristics one conversely to the other.

It is the same thing for classification. You can classify by adding the new classes through successive additions, that is uniting; or else you can classify by dichotomy, by dividing one class into two and so on. Uniting and dividing are the two characteristics *A* and *B*, and this is not merely theoretical, as is shown by the experiments on inclusion. For example, bunches of grapes, some white and others black, are put on the child's table and he is asked: 'Are there more grapes or more black grapes?' One observes then that up to about 7-8 years the child has great difficulty in including the part in the whole. Now it is easy to show that this including of the part in the whole presupposes just that connexion of two methods of uniting and dividing, and that it is a question of being able to pass from the whole to the part and from the part to the whole at the same time. As long as he can only make this passage in one direction and not in the other simultaneously he will make classifications which empirically are satisfactory, but he will not have the logical idea of inclusion.

I should like to add a final remark in connexion with Zazzo's reply to my paper. The substance of Zazzo's reply is: your process of equilibration seems likely in the particular case of the solution of specific cognitive problems, but does it apply in a more general way to development, acquisition and learning? Naturally I think it does. I will put the question in the following way: is an acquisition due to a system of associations or of assimilations? If it is a matter of associations it is simply that a piece of behaviour is repeated *n* times and it becomes stable by repetition: there is no problem of equilibra-

tion. But a difficulty arises: why do these simple repetitions give rise to liaisons which are stable in some cases but not in others? Everyone knows that this is the big problem of the conditioned reflex: Pavlov's dog will not salivate indefinitely if he only hears the bell and if he never again gets the food which should confirm that the bell has a significance. Conditionings which last are those which correspond to a need and those which are confirmed. In other words the association itself is not an explanatory principle; the association does not become stable in accordance with the number of repetitions but only in so far as it implies a need at the point of departure and a satisfaction at the point of arrival. When there is no satisfaction there is no stabilization. What is fundamental here is not the association itself, but the assimilation into the pattern. But as soon as assimilatory patterns appear we are faced with a problem of equilibrium. In fact all adaptation comprises two poles: assimilation to the structures and the previous activities of an organism, and accommodation to the present situation—and this brings up a problem of equilibrium. I think then that processes of equilibration occur from the beginning of development and that the pattern I have tried to propose to you is included in the theory of learning, being the particular case of learning without external reinforcements, without the 'reinforcement' of Hull, etc. Only the subjective reinforcement which is the pleasure of action or of understanding plays a part.

**LORENZ:**

I very strongly doubt that there is no reinforcement in this case; the simple matching of theory to facts gives very great satisfaction. You remember Harlow, the primate psychologist in Wisconsin, showed that in monkeys the solution of puzzles without the slightest reward was self-reinforcing. The solution was its own reward.

**PIAGET:**

Precisely. I said that there was no *external* reinforcement. There is an *internal* reinforcement through the pleasure of feeling satisfaction in having found a solution. But there is no external reinforcement, no means of objective control, no punishment nor recompense.

**ZAZZO:**

Professor Piaget, there remains for me a somewhat strange feeling: when you speak of equilibrium you make me think of Bergson when he speaks of duration—excuse me, but after all Bergson had a great mind, didn't he?—because Bergson considers duration as being independent and apart from the things which last (or durate) and I sometimes have the impression that you consider equilibrium independently from what is in equilibrium. For you it is a factor

which is added to and combines with the others but seems not to be resultant of all the material causes which can act. In other words you frequently give the impression of explaining the logic of evolution by the evolution of logic!

PIAGET:

As regards this particular case of logical structures, I should like to get rid of a misunderstanding which seems to me to persist in this discussion. I have not the slightest desire to generalize from the case of logic to all the rest of mental life. Logic is the only field where equilibrium is fully achieved; in the other fields we are faced with partial equilibria which do not reach the measure of operation but only that of regulations and feedbacks. If I had had the time I should have spoken of perception, and shown you that one finds exactly the same processes: strategy 1 is a focusing, strategy 2 another focusing, strategy 3 is the passage between the two, the defocusing, etc., but I should particularly have shown you that in all the perceptive mechanism we studied we found the first three strategies but never the fourth; the fourth appears to be peculiar to logic.

Logic is a particular case but, on the other hand, as far as the first three of its strategies are concerned it is a case which is a part of a much wider category; of a category of all the phenomena where processes of retroaction and anticipation occur, which is almost all vital phenomena. That is why I think that the pattern of equilibrium is a very general pattern but is achieved in its complete form with strategy 4 only in the field of logic amongst cognitive functions. When Zazzo says that my theory of development is actually a history of logic he goes beyond my thinking: but logic is a specially instructive case because everything is found there, whereas in all the other fields one finds only regulations, near-reversibility, semi-equilibrium, etc.

LORENZ:

I want to say that a sigh of relief was audible at this part of the table when you said that complete equilibration was only achieved in cognitive functions. Much as I am in agreement with this thesis, I think that nevertheless I would add a type of function which is not usually thought of as a cognitive function, but which at least is analogous, and that is the phenomenon of perceptual constancy. You say that complete equilibration never occurs in perception—but I believe it does. Let me give an example—if I move my eyes to and fro this room doesn't seem to move, it remains stationary in my perception—we don't see the walls wobble. And you know that the explanation of this non-production of subjective illusion is due to a very complicated regulating mechanism only lately discovered.

The principle of 'reafference' plays a most important part in the physiology of perception, and I might explain in a few words what it is. If I turn my eye sideways passively, by shoving a finger into the eye-socket, I do get the illusion of the environment moving in the opposite direction. If, on the other hand, we fix the anaesthetized eye, by pressing a little ring to the eyeball, so that it cannot move, and then try to look to our right, the whole room about us seems to pivot in the direction of our intended eye movement. This is the important point: that we have not actually moved the eye, we have only sent out the voluntary motor impulses which would have moved it under normal circumstances. It is the sending-out of the motor impulse which creates, all by itself, the illusion of a movement of the environment in the direction of the intended eye movement. This illusory movement is the exact reverse of what happens to the retinal image when the eyeball is allowed to move freely. The illusory movement caused by the impulse itself, and the real retinal movement caused by the turning of the eye, counteract and extinguish each other completely. If we move our eye to the right, of course our retina shifts in the opposite direction. We should see our environment moving correspondingly, if it were not for this process of compensation, in which the movement of the retinal image is extinguished by an 'illusory movement'. In order to make all this more intelligible, I might describe the same process in an anthropomorphic parable, putting a reasoning human mind in the place of the mechanisms of perception—which Helmholtz did quite seriously, when he interpreted this kind of process on the basis of 'unconscious reasoning'. The moment the voluntary command to turn the eye to the right is sent to the muscle, a correlative message is dispatched to the visual cortex, telling it what movement is to be expected, in consequence of the impulse, from the retinal image. Thus the central receptor is warned not to interpret as a movement of the environment what really is caused by a movement of the organism. The consequence of the message to the central receptor was termed 'Reafferenz-Erwartung' (Reaffection Expectancy) by Holst, a term which he later dropped because of its anthropomorphic quality. This message paralleling the motor impulse is now termed 'Efferenzkopie'.

The principle of preventing an illusion by actively creating another one counteracting it is even more widely found among the processes of perception than is the principle of reaffection. Let me give you an example of this. If we stand before a long and high wall and look at its straight and horizontal upper edge, we see it as a straight line. But, in consequence of perspective, what really is projected on our retina is a wide arch with its convex side upwards. If, in complete darkness and without any other points of reference, we look at an equally long and straight horizontal luminescent line presented above

the level of our eyes, we do perceive it as a wide arch with its convexity upwards. It is this 'illusion' which enables us to perceive straight horizontal lines as straight when they are above, or, for that matter, below eye level, in spite of the fact that in this position their retinal image is distorted by perspective.

All these mechanisms of constancy are very much akin to cognitive functions in that they 'abstract', out of the information contained in the sensory data, properties which are constantly inherent in the perceived object. One of the chief functions of perception in general and constancy effects in particular is true objectivation, just as is the case with cognitive functions.

I think that constancy effects are attained by a process of equilibration which, in some of the instances I cited, is just as complete as in cognition. And I think that this exception from the rule stated by Professor Piaget is one that actually proves the rule, because constancy perception is functionally so closely akin to cognition in its objectivating function that it might also be subsumed under the conception of cognitive processes.

**PIAGET:**

I am delighted with what Lorenz has just said. Firstly, when I was using the term cognitive functions I was not thinking only of intelligence but also of perception. Secondly, in perception it is of course the constancy phenomena which are closest to the phenomena of conservation. It is the same thing on another scale; for years we have been working on the relation between the perceptive constancies and operatory conservations, and I entirely agree with everything Lorenz has just said. Consequently I think this shows the general nature of the problem of equilibrium, it being understood that logic remains the only cognitive field where complete equilibrium is attained.

In fact, as regards perceptive constancies, can one actually speak of a strict total equilibrium? If it is true, I would say that there is a logic of perception. But in all the cases we have studied experimentally up to now we have found a mechanism which is analogous with that of conservation, but for a constancy which is not absolutely strict. I will give you as an example the constancy of sizes in depth (Piaget & Lambercier, 1951). The rule in the adult, among hundreds of subjects that we have measured now, is not absolute constancy but a super-constancy, that is to say that a stick of 10 cm shown four metres away from the subject is seen by the adult as being rather larger than 10 cm. The average is about 9.5 cm for it to be seen equal to 10. In other words, we have a regulatory mechanism which goes beyond the exact balance, an anticipatory mechanism which is a precaution taken against error. If you wish, it is almost logic, but

this little excess shows that it is still regulation and not quite operation.

**GREY WALTER:**

I want to emphasize what Konrad Lorenz said. Recently there have been some disturbing discoveries made about perception in intact animals which, in effect, show that there are mechanisms within the brain which act like traffic cops for incoming information and actually damp down and modify the action of the receptors themselves. It has been shown that the information which is allowed to reach the brain from the outside world is a function of its novelty and significance. The level of the receptor itself, the actual eye or ear, is cut down, as though the central nervous system were to say: 'I'm not interested in what you're sending me.'

The recent work by Hernandez-Peon and Jouvet (1956) on the ear seems particularly clear. The point is that the information reaching the central nervous system is even more corrupt than we thought before, not merely because of differentiation or fatigue or adaptation but also because of this very positive traffic control system.

**MONNIER:**

I do not think that the physiological experiments of Granit on sensory organs really support the conception of compensatory illusions expressed by Lorenz. They prove only that the receptivity, the 'Bereitschaft', of the sensory receptors is regulated by efferent fibres. By virtue of this mechanism the threshold of the muscle spindle may be lowered and the muscle receptors may become more or less ready to detect external or internal stimuli. This occurs for example in the knee-jerk reflex, which may be reinforced by a voluntary movement of the hands.

**LORENZ:**

Similar mechanisms of analogous function but obviously working at a higher level of integration exist also in the central nervous system. My point is that all of them work on the principle of creating a compensatory illusion in order to exclude unwanted information. When you stand for some time looking at a lake on which the waves are all travelling quickly to your left and then raise your eyes to the opposite bank, you see, to your surprise, that the bank seems to be gliding towards your right. And if you have been driving a car for a very long time, and then stop, the landscape seems to float smoothly backwards. This is always my illustration for the fact that we cannot, on principle, distinguish between the illusion created by central compensatory mechanisms and the real sensory information: again and again, when suddenly forced to stop before a railway crossing

after a long drive, I felt my car sliding smoothly backwards and said to myself: 'Aha, that is the well-known illusion'—and did not do anything about it, until the man behind me started blowing his horn in utmost alarm because I really was rolling back on him. Your laughter shows that the phenomenon is known to most of you! The point it proves is that even if you are interested in making the difference between sensory information and compensatory illusion, you simply cannot do it.

I think that failures of constancy mechanisms in really achieving constancy, or in overdoing it, are quite frequent. There is one example we found in making animal films which concerns a failure of time constancy. If, in a film, you show a cat licking her kittens, first taken from a distance, and, immediately after that in a close-up, the close-up must be slowed down quite considerably, else you get the unavoidable impression of a speeding-up of movements with the enlargement of the picture. If the face of a kitten covers most of the screen and the mother's tongue travels, with each stroke, from lower left to upper right, our constancy effect breaks down and we see that tongue travelling at enormous speed. We cannot correlate speed to size correctly and in order to counteract this phenomenon one had to take all close-ups of quick movements in a slow-motion of a measure nearly equal to that of the enlargement.

The more complicated constancy phenomena seem to be much less precise in most animals than in Man. That's a very sweeping generalization, and in swift-moving animals, of course, size-constancy must be very well developed. On the whole, though, complicated Gestalt-constancy is much less good in animals than it is in Man.

#### GREY WALTER:

There is still some doubt in my mind as to whether these stages of intellectual development are inevitable stages or simply the results of experience. It is very important to know whether development by these stages occurs because the machine simply matures that way, or whether it is due to the gradual acquisition of sufficient information from the environment finally to permit, for example, abstractions of action. Professor Piaget, do you think these changes of thought in children are the result of the accumulation of redundant information from a large amount of experience or do yourself in your heart think that these are stages which will develop inevitably even without accumulation of information?

#### PIAGET:

The problem is to know whether a progressive nervous organization occurs or whether an accumulation of external information is

sufficient. I do *not* think that one can explain intellectual operations simply by the accumulation of information, because there is not just information there, there is the way the information is structured and co-ordinated.

GREY WALTER:

I am not suggesting that the truth may lie entirely on one side or the other, but one would like to know the proportional importance of learning and maturation. To what extent are there inevitable, self-sustaining processes in the nervous system which make the child ready to walk, ready to talk, ready to perform these various cognitive or affective operations, irrespective of his experience? Or to what extent is it all modifiable or modified by experience?

FREMONT-SMITH:

Coghill, you remember, said *experience is growth* as far as the nervous system is concerned. One might say there are two inter-dependent aspects of development of the nervous system. One is the growth in terms of size and complexity at the more macroscopic level, and the other is the structuring and complexity resulting from experience. Perhaps you really couldn't get a fully developed brain without information being received by it.

GREY WALTER:

I think Coghill's data are a little bit facile, really. For example, there is information now from the comparison of premature babies and mature children born at full term (Douglas, 1956). The prematures are a month old at the time most children are born, but there is no difference from full-term children, in the time at which they walk, calculated as post-conceptual ages, despite their having had a month more of extra-uterine experience. It looks rather as though walking is a more or less inevitable development, a maturational growth process.

LORENZ:

I myself am quite convinced that in the interaction of maturation and learning in the progression from stage one to stage four, there is very little carry-over of learning from one experience to the next and that it is, say, 3 per cent learning carried over from previous experience and 97 per cent maturation process in the central nervous system. My impression is that if you should make a large series of experiments with children who are daily faced with these problems and children who are faced with them for the first time at a given age, you would find as little difference as my pupil Grohmann found with pigeons prevented from making flying experiences and those that were not (Vol. I, p. 52).

BERTALANFFY:

I have certain misgivings about speaking of 'factors of development' even though they are said to be 'interacting'. I think you cannot say there are hereditary and environmental 'factors' as if they could be isolated. With a slight exaggeration you might say that everything is inherited and everything is environmental. It is a truism for genetics that what actually is inherited are not characters but what Woltereck has called *Reaktionsnormen*, i.e., potentialities to develop certain characters, given certain environmental conditions. Perhaps the best illustration is what the geneticists call penetrance. There are some characters like the blood groups which show up under any circumstances if the organism develops at all; there are also characters which even though they are genetic and inherited manifest themselves only under rather special circumstances or in an irregular way. Similarly, there is no 'equilibrium factor' in physical systems; there is only a constellation of forces which may (or may not) lead to equilibrium.

The 'equilibrium factor' in Piaget's sense is, as I see it, nothing else but the establishment of the categories of cognition. It is the unique and profound contribution of Piaget to have shown us how these develop. While, according to Kant, the forms of intuition and categories of experience were supposed to be *a priori* and universal, we realize, to a large extent owing to Piaget, that cognition and experience of the world do not follow hard and fast rules. Rather the categories of experience depend on the one hand on the psycho-physical organization of the living being concerned, and on the other hand on linguistic and cultural factors.

For example, the category of substance, which is unavoidable and obvious to our way of thinking, seems to be connected with our linguistic habits, the constructing of sentences with substantive and adjective in the Indogermanic languages, which led to the Aristotelian distinction between substance and attributes (see Bertalanffy, 1955a). In languages like Hopi or Chinese, there are quite different categories to bring order into our experience (Whorff, 1952). Thus it appears that the categories which have been considered to be absolute and *a priori*, in fact are different in different cultures and are slowly formed during the mental development of the child. I would think Piaget's equilibrium factor would have a different course of development in different cultural and linguistic conditions.

LORENZ:

I agree on the principle that *some* ways of thinking are dependent on verbalization. Existentialist philosophy, for instance, could never have developed in a culture which was not in command of the verb 'to be', it is a deification of the German 'copula', the auxiliary verb

'to be'. But I emphatically disagree with the belief that verbalization has to do at all with the category of substance; the development of the notion of substance is connected with the development of perceptual constancies.

BERTALANFFY:

But you remember Charlotte Bühler (1930) has shown that perceptual constancies themselves develop during the maturation of the child, the error in estimating size, for example, being considerably larger in the two-year-old child than in the three- or four-year-old.

As far as verbalization is concerned, I think one of the difficulties preventing us from arriving at satisfactory theories in matters involving psychology is our dependence on our linguistic bondage. Our language describes psychological happenings in physical metaphors. For this I quote two different and excellent authorities. One authority is Konrad Lorenz himself who, in 1943, emphasized that our way of dealing with psychological experience is based upon physical similes. 'Comprehend' means 'grasping something'; a 'connexion' is a physical link between things; even 'time' is represented in terms of visualized space when we speak of a certain 'span' of time, and so forth. My other authority, from quite a different field, is Benjamin Lee Whorff. From the view point of anthropology and linguistics, he said essentially the same thing, namely, that we are using spatial or corporeal metaphors in order to express psychological experience. What applies to our way of expression in the vernacular, also applies to the models we use in science. When you speak of 'equilibrium', 'homeostasis', 'open system', and so forth, it is always a physical metaphor which is used. Freud, in his 'dammed-up libido', and Lorenz's concept that instinctual energy is built up and discharged if a certain level is surpassed (even without stimulus as in *in vacuo* behaviour), both use the same hydrostatic model. Again, it is physical models that psychoanalysis uses in concepts such as incorporation, intrusion, elimination, and the like. Unfortunately, within the structure of our language and thinking, we can hardly do otherwise, and can only try to make the best of it.

This, it seems to me, is one of the reasons why psychological theory is so difficult. It might be easier, for example, in the Hopi language where there are fundamentally different categories to bring order into experience. There are no tenses. They make no distinction between present, past, and future. Rather the distinction is between what one may call the manifest, that is, all that is accessible to sensory experience, and the un-manifest, which comprises both the future and what we would call mental. So the future and the mental processes go under the same category. Such language would lead to the

formation of categories which are impossible, or very difficult, to approach from our standpoint.

MEAD:

You must remember that this doesn't mean at all that people who use such different categories can't build a house, that they don't know the difference between yesterday, today and tomorrow. The most striking example is the Trobriands where there is no recognition of causality of any sort in the language. A seed never turns into a plant that turns into a larger plant. You have a seed, then you have a plant and then you have a larger plant as if they were all coming preformed from somewhere. The Trobriands refuse to recognize physical paternity, they refuse to recognize that the food goes into the body and is added to it. The body is simply a tube and you take the food in and you excrete all of it and nothing from the food goes to you. But this doesn't interfere with their having an adequate marriage system, disapproving of illegitimacy, making roads that go some place and building houses that stand up. All of these observations of the external world, which we have disciplined into a system, they leave in an implicit non-verbalized state and all the things that we put into a dream state and only permit to poets they put into a system.

On the other hand the Manus child would be dead if it made the mistakes in real life that some Swiss children make in the experimental situation of Piaget's tests. They don't think that when they change the shape of something they change its weight. If they did they couldn't handle their canoes, and load things from one to another. They have to know *in activity* all these things that they are only able to conceptualize much more slowly. Progression in Piaget's tests seems to me a process of maturation meeting with a process of teaching and conceptualization which is culture-bound. Let me give a complementary illustration. There are people who mature in dreaming, as we mature in our ability to understand logic and use mathematics. There has been a study made of three different primitive people with three different levels of dreaming. The children's dreams were alike in all three of the societies, and showed no formal differences. But in one society you get a second stage where you can use your dream as a directive for yourself on how to get well and things of this sort; and in the third society (the Penang of Malaya) there is a higher level of 'maturity' in which you can turn your dreams into useful social instruments. Here there are schools of dreaming, young people sit around and their dreams are criticized, and they gradually mature to the stage where they can use their dreams in a different way. Now, if we had precise and sensitive techniques to study this capacity I think we would find a series of steps of maturation

with age in it too. But the society that can teach people how to dream constructively would reflect these maturational stages differently from ours. I might add that we can't do this, and one of the reasons we can't do it is probably because we are so busy teaching children to carry out the type of logical thinking which Professor Piaget is examining.

### THIRD DISCUSSION

## The Definition of Stages of Development

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#### PIAGET:

We are now going to discuss the problem of stages of development. I will not repeat what is in my paper (in Part I); I shall merely reply to the objections which were made: not for the purpose of defending myself but in order to raise problems of general interest.

I will begin with Margaret Mead's reply, in which she maintains—and I entirely agree with her—that the stages cannot be characterized by average chronological ages, since these average ages, of course, vary considerably from one social environment to another even in the same society, and still more from one social culture to another. On the other hand, she agrees with the criterion of the order of succession, and in particular the criterion of successive equilibrations, and shows that a series of researches could be undertaken in this field to give precision to the stages and their implications. Therefore I should like to put before her a programme of possible research with the collaboration of several different members of the Group if possible, but in any case between Margaret Mead and ourselves.

The research is to determine how far certain stages are found in all cultures, and other stages not found. I will simply give four examples in the field of cognitive functions; the examples show at the same time that even in the cognitive field the value of stages can differ greatly according to the structures involved.

1. The first comparative experiment on stages has to do with the reversible operations of which Bärbel Inhelder and I have often spoken, but operations benefiting from the help of perceptive configurations in a field where perception is not in conflict with operation. A good and a very simple example which according to our forecasts should be found in the most different cultures is seriation. A child is given a certain number of little sticks in disorder to put in order from the smallest to the biggest. They must be sticks which are not too different, because if they are too different it is simply a matter of perceptive reading without reasoning. You need, for example, sticks differing by about half a centimetre in 10 to 20 centimetres

to force the individual to compare them two by two. Here in Geneva we find three stages (Piaget & Szeminska, 1941). There is a first stage where the child puts together a small and a big stick in pairs, which he does not manage to co-ordinate; or else he makes little series of two or three, but he does not manage to co-ordinate the series. We find a second stage where the child proceeds by trial and error; he begins by making series which are fairly but not quite regular, then he corrects them and finally he manages to make the exact series. We find a third stage—and this for us is the criterion for acquiring the operation—when the child takes all the relationships into account from the beginning. He looks for the smallest stick and compares it with all the others. When he has found it he looks for the smallest of those that remain, and then he puts it next to the first, then the smallest of all those that remain, and so on. In other words, he understands the relations according to which any element is at the same time both larger than those already put down and smaller than those which remain.

Now do we find these three stages in different cultures? This is a very simple experiment. This year Bärbel Inhelder and some collaborators carried out a very interesting variation of it by asking the child not to handle the sticks straight away, but to anticipate by mental imagery, by language, and especially by drawing, how he is going to make his series of sticks (he is given sticks of different colours with different coloured pencils to see how he can solve it by drawing). We then got the result, that at stage 2, that of empiric trial and error, certain children are capable of anticipation: when it is a matter of making a plan before handling the sticks they make an absolutely correct drawing. They say to you: 'I am going to put them like that', (and when the drawing is in black it is fine; when it is in colours it is the same problem as with the manipulation itself, it is more difficult). Nevertheless, the child does not afterwards manage to make the series in an operatory fashion because the anticipatory pattern is not an operation, it is a progression in a single direction. In the operatory system he must take into account the two relationships of going up and going down which are not found in the anticipatory pattern, which is only a configuration based on perception.

2. For the second experiment I will take an operation again, but without the assistance of perceptive configuration and on the contrary in conflict with it. Here we can think simply of pouring beads from one vessel to another (Piaget & Szeminska, 1941). We have two jars of the same dimensions and the child himself has put a bead into one of the jars every time he has put one into the other, until he has ten on each side. He thus knows there is the same number in each jar. He then pours the beads from one of the similar jars into a jar of a different shape and is asked whether there are as many, if there

are more, or less, etc. We find three stages: the little ones deny conservation, they will tell you that there are more because it is higher, or that there are less because it is thinner; in a second stage there is conservation for smaller transformations but not for large ones; and during a third stage conservation is affirmed as being evident and necessary.

3. Thirdly, we can take an example of pure operation, but with the assistance of language. The question is: is 5 equal to  $3+2$ , using the same technique of correspondence between two series, but with the possibility of counting (Apostel *et al.*, 1957). Language can here assist the investigation of the elements and favour conservation.

The experiment is carried out as follows: the child is given 5 red and 5 blue counters, each red one paired with a blue one. He knows that there is an equality, that there are as many blue counters as red counters. Then the red counters are displaced, but without the length of the row or the arrangement of the parts being altered. Generally the child agrees that there is still equality. Then this collection is changed into two sub-collections of 3 and 2 and the child is asked whether there is still the same number of counters (I am speaking here of small collections of 5, but of course the experiment can be done with 15, 20 or 30 counters). We find then the following stages. The youngest subjects that can be questioned (about 4 years old) say 'it isn't the same thing any more. Now there are more because you have two packets, while over there there is only one packet.' Then you find children will begin to count in order to verify, but an intermediate stage is observed which is very strange, where the child counts but is not convinced by spoken numbering. I remember a child, for example, who said '1, 2, 3, 4, 5 and 1, 2, 3, 4, 5. Well, I find 5 on each side but there are more here' ( $3+2$ ). This reaction disappears later. The child uses counting in order to verify an equality and he admits that there is an equality of quantities of sums when there is an equality of numbers, but only up to a certain number. Another child, for example, admitted that  $10=8+2$ ,  $12=8+4$ , that  $15=10+5$ , and for 16 it was still all right, but for 17 he began to count and said 'Well, I can see there are 17 here and 17 there, but now it is not the same any more.' In other words he goes back here to behaviour left over from the previous stage. Finally, at the last stage, the child no longer needs to count. He says 'Of course, it will always be the same thing, it is the same collection, you have just divided it in 2 or 3 parts,' etc. 'It is not worth while counting, I know, I am quite sure.' So the final analytic process is the result of a development where we find all the transitions. By questioning a few more children, I discovered even finer transitions.

4. Fourthly, I will take a problem of representation without operation; of simple representations with language but without any

logical problem. For example, the questions of animism (Piaget, 1929) which I think Margaret Mead has already dealt with among children in New Guinea.

According to our forecasts, which are of course purely hypothetical, I think that the first experiment, that of seriation or operatory configuration with the assistance of perceptive configuration, must be most general and must be found in almost every culture. Experiment 2 is perhaps a little less general; experiment 3 is certainly less general because spoken counting comes in, and counting is a collective and cultural technique. Then in the fourth experiment (animism, etc.) there are no operations and I think therefore that there is no reason why there should be uniformity of reactions from one cultural environment to another. In this last field I do not think that one would find general (that is cross-cultural) stages.

This is, of course, only a very short sketch. It would be necessary to study much more closely a table of operatory tests. I have given this simply as a basis for ideas and to furnish concrete examples.

MEAD:

When I said that I didn't think discussing the average performance of a child at some designated stage was very much use, Professor Piaget interpreted that as a statement about a difference *between* cultures. I only meant it partly as that; I meant it even more importantly as a difference between individuals *within* a single culture. It seems to me that an average for any stage obscures most of the things that are interesting and important. Each culture differentially selects from among the available types or styles or ways of learning which are potential in any reasonably large sample of the human race. For example, there are some societies that won't let a child walk before it talks and others that won't let the opposite happen. So a certain group of children will be held back until another group is ready to catch up with them, and a great deal of social pressure will be expended to obtain uniformity at a point where a uniformity would not otherwise exist. Thus a child may not be permitted to crawl till it has a certain number of teeth.

Another gross social intervention amongst the varieties of maturation which I think makes an enormous difference in the end is saying in a given society that a child can't get to school until it is six. If children are taught to read at four, which is quite possible, and used to be done, then those children will approach language and every other problem differently from those that don't strike any type of symbolization either in mathematics or in reading until, say, eight or until they are adults.

Thus I see the significance of cultural intervention in biological development as only explicable if one includes individual differences

in patterning. These differences include evenness and unevenness of physical maturation which we're beginning to know a good deal about, and include the fact that some children seem to be able to come into a particular period much earlier than others. When Professor Piaget described the experiments that he was suggesting that he and I do together, he said 'I remember a child who . . .' and then he mentioned the particular one who could do the sixteen and not the seventeen items. That child disappears in a general statement of stages—whereas actually I think every one of the differences amongst these children is significant. If we think only cross-sectionally, and leave out these individual differences, then in effect we leave out the cultural differences also.

Piaget presented these four experiments saying that he expected when you got to the verbal level that the tests would not be done in different cultures in the same way, whereas at the perceptual or the manipulative level he expected great similarity. But that leaves out of account what cultures actually have done. Some cultures have emphasized one of these levels and other cultures another. There are cultures in which people can do a great deal of manipulation of language, but will not be able to count beyond 20; there are others where people can count to 400,000 but are bored with language and do almost nothing with it. My comment on the question of averages was designed to include these two kinds of differences. Individual personal differences—whether they're seen as types or as patterns—have almost disappeared in the papers that we wrote in Part I although they've been present in our discussion over and over again.

The second point that I wanted to make was to welcome this suggestion for cross-cultural co-operation on a group of children. I think this will be a cumulative process during this meeting in which each one of us will have ideas of co-operation that ought to build up by the end of this session to some plan by which the different approaches can be grounded in identified living children, and not remain floating around in the air as attributes of something called 'the child'.

#### PIAGET:

I will now pass on to Zazzo's objection which is connected with the definition of stages. Here I should first like to remind you that in my paper the definition of stages is simply a reproduction of what Bärbel Inhelder said in our first meeting (Vol. 1, p. 84), when she attempted to find criteria for the stages. She indicated five criteria: (1) the order of succession; (2) integration of the acquisitions at one stage into the following stage; (3) a whole structure characterizing the total aspects of a stage; (4) the fact that one stage prepares the following; and (5) that the new stage constitutes the culmination of

what is prepared in the preceding stage, in other words an equilibrium step. Now Zazzo replies that these definitions are too limiting, and that naturally if one defines the stages in too limiting a way it will be easy to say afterwards that there are no general stages. He proposes, therefore, a much more elastic definition of stages.

I will simply reply that these are degrees of the possible structuration of stages, and, that, of course, one can be content with one or two degrees within the five. One can very well conceive of a series which can be called stages which would conform only to the first criterion, that is to say, to the constant order of succession. One can very well imagine another series of stages where there would be at the same time order of succession and integration. One can very well conceive of a third series of stages where there would be the first three requisites and not the two last, etc. I do not think that these criteria constitute a kind of *a priori* framework. The criteria I have presented are what can be found in a field where stages are clearest; there is no question of generalizing these requirements for all stages in all fields.

ZAZZO:

That reply seems to me very satisfactory because it admits differing definitions for different levels, and I think that links up with what Tanner said in his paper.

PIAGET:

I now come to the big problem: the problem of the very existence of stages; do there exist steps in development or is complete continuity observed? Now, Tanner, upholding the position which he has adopted during all the meetings of our Group, shows that in somatic development one finds only continuity. He therefore naturally tends to generalize this idea and in opposing the idea of relative discontinuity in other fields he makes an objection which seems to me extremely serious: it is that when we are faced macroscopically with a certain discontinuity we never know whether there do not exist small transformations which would be continuous but which we do not manage to measure on our scale of approximation. In other words, continuity would depend fundamentally on a question of scale; for a certain scale of measurement we obtain discontinuity when with a finer scale we should get continuity. Of course this argument is quite valid, because the very manner of defining continuity and discontinuity implies that these ideas remain fundamentally relative to the scale of measurement or observation. This, then, is the alternative which confronts us: either a basic continuity or else development by steps, which would allow us to speak of stages at least to our scale of approximation.

I think Bertalanffy's paper brings to this point documentation of great value. Bertalanffy takes his examples from fields as different as architecture, the history of automobile technique and the evolution of species in order to demonstrate that this is a general problem. In the evolution of species it is remarkable that one has great difficulty in finding intermediate types between two species. Stable species are found, as if there were no evolution; one observes only very few transitional forms which would enable evolution to be demonstrated. Thus the solution proposed by Bertalanffy, using the ideas of equifinality and equifinal steps, is that the intermediate types are unstable, whereas the species themselves are steady open states. In the case of species there would, then, be equilibrium steps for certain morphogenetic organizations, whereas the cases of transition would be unstable.

This conception of Bertalanffy's seems to me to correspond exactly with what Inhelder and I have found in the field of cognitive stages. On the one hand we find stages which characterize a certain proportion of individuals at any given age. On the other hand, we always find sub- or intermediary stages, but as soon as we try to pin these intermediary stages down we enter a sort of cloud-dust of sub-intermediaries because of their instability. Other organizational steps are relatively more stable and it is these that one can consequently consider as 'stages'.

The usual reproach made about stages like those I have been reminding you of is that in making them continuity is neglected. This is exactly Tanner's argument; and generally, every time we constructed a series of stages in one field or the other of the cognitive functions, Inhelder and I came up against the same objections of exaggerating a discontinuity and neglecting a continuity which we might have found if we had made a finer analysis.

This year, however, in the work of our Centre for Developmental Epistemology in Geneva I met with the reverse objection from one of our collaborators; that is to say, he systematically saw discontinuity everywhere where I tried to show him transitions and continuity from one stage to another. The problem was one which interested our Centre and which goes perhaps rather beyond the interest of psychologists: it was the problem of what is called in logical empiricism the analytic and the synthetic. The question was to establish whether there exists between analytic and synthetic statements a complete opposition (Carnap) or a continuity of transition (Quine). It was for this reason that we carried out the psychological experiment (amongst others) on counting and verifying  $3+2=5$ , that I described above. The idea was to take a statement considered by all the logicians to be strictly analytic, but which I

wanted to show actually began in the child by being experimental and consequently synthetic.

However, despite this, my collaborator maintained that if the changes were slight they were nevertheless always made by sudden reorganization and discontinuous steps. He was a logical empiricist who insisted on the distinction between the analytic and synthetic, and I did not manage to convince him that there was continuity. In another field I should perhaps easily have convinced him. In other words, these notions of continuity and discontinuity depend not only on the scale of measurement but also on the general system of interpretation.

I will conclude, then, by saying that stages of development appear to me to be a reality, but differ from one field to another; they are more or less defined, more or less precise and accentuated according to the fields. As regards general stages common to all fields of development, I am in some doubt. I can neither affirm nor deny their existence. One can only decide by successive approaches, which would consist in establishing a series of correlations, correspondences and parallelisms—work which is almost entirely still to be done. Lastly, the idea of stages seems to me necessarily linked with the idea of equilibrium, or steady states.

GREY WALTER:

The definition of 'stage' is essential to our whole philosophy about children. We can't escape the general axiom that in dealing with an organism there are bound to be thresholds; there are bound to be stages below thresholds at which nothing happens, stages above thresholds when something does happen. But the difficulty arises when one has many thresholds, that is many processes each with a different threshold, in which a series of step-functions may overlap. Piaget seemed to me to be saying that in studying the *whole behaviour* of a child, there may well be so many thresholds that the various jumps superimpose and produce a smooth curve. It seems to me that one of our tasks is to see if we can dissect out the various step-functions and thus isolate particular psycho-physiological functions in the child for further investigation. The origin of some of our misunderstandings may be that some of us are looking at individual functions—for example, the existence of a particular type of brain activity in the EEG, or the existence of some drive in sexual behaviour—whereas others are looking at whole behaviour in which there is a blurring of these otherwise critical stages of development.

TANNER:

I was going to say very much the same thing. Piaget acknowledged the force of the argument that something which appears as a stage

may do so because it has been measured on a coarse scale and that if the scale is made finer one may discover there is continuity. In my turn I agree that if you go still further down in refinement, down to the cellular level, you again come to step-functions. Thus there are some behavioural levels where stages appear, other levels, for example of physical measurements, where continuity appears, and then the cellular level where ultimately there is again discontinuity. It is chiefly for this reason that, as I said in my comment on Piaget's paper, I don't feel the question of stages is a very real one. I think that stages are useful only in certain operational situations and that it would probably be a mistake to discuss as a question of philosophic importance whether stages do or do not exist in the disembodied or decontexted sense.

**PIAGET:**

I should like to make a quotation. It is from Henri Poincaré and he said 'Scientific research consists in making what is discontinuous continuous and in making what is continuous discontinuous'!

**INHELDER:**

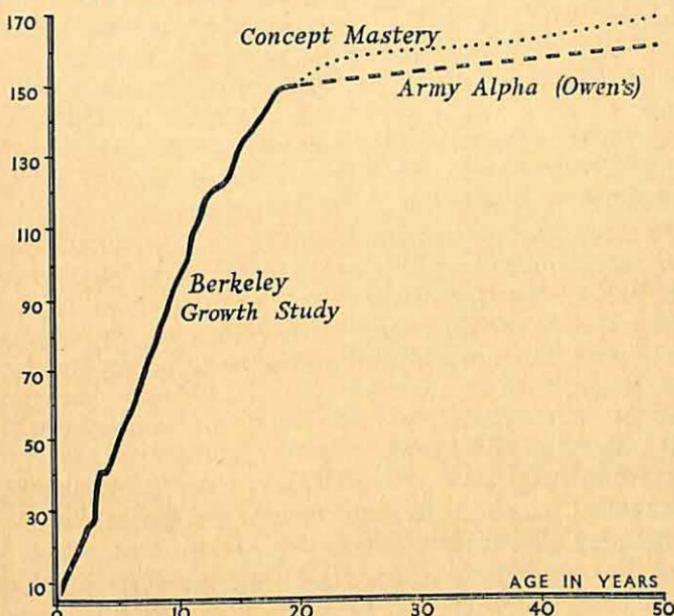
Certainly in the field of cognitive functions stages have no absolute significance. What actually does a stage signify if not a change of a qualitative order, a sort of metamorphosis? We can speak of stages only when we observe a real change in behaviour in a defined situation. Such a transformation can be marked by a change in the way of thinking. It may happen, for example, that a child for a shorter or longer time tries to find a solution to a problem by the method of trial and error. He gets close to the correct solution through a series of successive adjustments, without, however, achieving success. Then almost abruptly he changes his tactics and reasons in a perfectly logical way with a feeling that it has become self-evident. It is then that we are faced with a new form of thought qualitatively different from the previous form. This new method or tactic will itself serve as a basis for new trials and errors in connexion with more complex problems. This metamorphosis of thought is particularly clearly seen in respect of ideas of conservation.

**BERTALANFFY:**

Here a question occurs to me. We have the stages as analysed by Professor Piaget. On the other hand, we can compare the curve of brain growth (Vol. I, Fig. 1, p. 37) with the 'age curve of intelligence' (see Fig. 1) based on the Berkeley Growth Study. It seems that these curves of brain growth and mental growth show a remarkable similarity. Would you say that the smooth Berkeley curve is caused by a smoothing-out of Professor Piaget's stages?

FIG. 1  
A PROPOSED AGE CURVE OF INTELLIGENCE,  
BIRTH TO 50 YEARS

Based on data from the Berkeley Growth Study, the Terman Gifted Study, and Owen's Iowa Study (after Bayley, 1955).



INHELDER:

Yes, I suppose it is a cumulative effect.

The effect of pseudo-regularity in curves of development can result from the way the tests are grouped. It has been noted that different ideas of conservation, of an arithmetical, geometrical, physical, etc., order are not formed at exactly the same moment. The processes involved are parallel, but not synchronous because one finds more difficulty in structuring one aspect of reality than others. These overlaps are one of the reasons why for many years I refused to believe in general stages of cognition. It was only later on after I had noticed the surprising concordance of structural order in the mechanisms of thought that I was able to show steps within cognitive functions. These steps are characterized by structures.

These structures in the concrete thought of the child or in the formal thought of the adolescent always represent the *optimum* of his operatory capacity. Naturally, during each day the child goes through oscillations of thought, and both the adolescent and the adult are far from reasoning formally all the time. The attainment

of a cognitive stage merely means that an individual under optimal conditions becomes capable of behaving in a certain way which was impossible for him before.

**PIAGET:**

This is the same point that Bowlby raised when in his reply to my essay he said 'I wonder if Piaget accepts the idea that, at all ages, behaviour is regulated by cognitive processes of different degrees of development—that in some of our actions we operate with a fully-fledged intelligence and in others none at all, and that in respect of any one activity we may shift from one level to another?'

Well, I fully accept this idea. Our cognitive functions are certainly not uniform for every period of the day. Although I am mainly engaged in intellectual operations, I am for example at an operatory level for only a small part of the day when I devote myself to my professional work. The rest of the time I am dealing with empirical trial and error. At the time when I drove a car and my engine went wrong it was even empirical trial and error on a very low level, as you can imagine. Every moment I am indulging in pre-operatory intuition. At other times I go even lower and almost give way to magical behaviour. If I am stopped by a red light when I am in a hurry it is difficult for me not to link this up with other preoccupations of the moment. In short, the intellectual level varies considerably, exactly like the affective level, according to the different times of the day, but for each behaviour pattern I think we shall find a certain correspondence. For example, for a primitive emotion a very low intellectual level, and for a lofty aesthetic or moral sentiment a high intellectual level. We shall always have this correspondence between the two aspects.

**BERTALANFFY:**

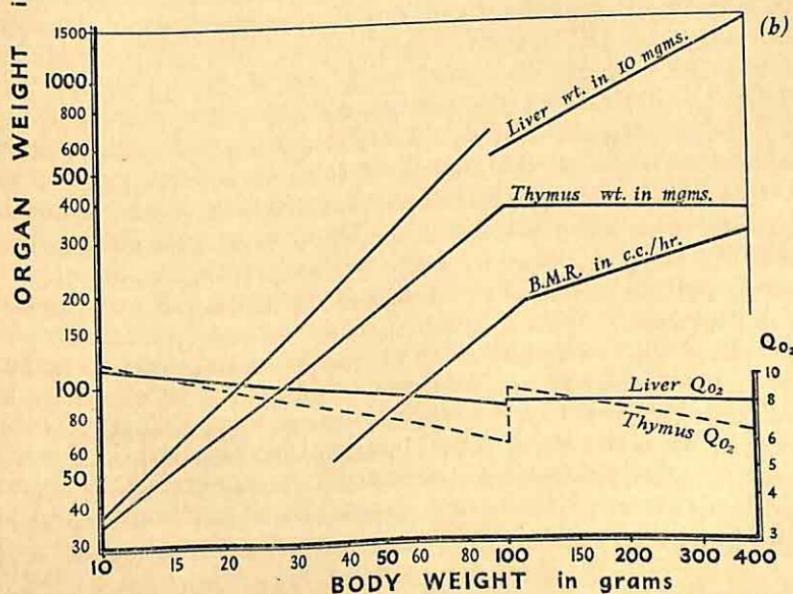
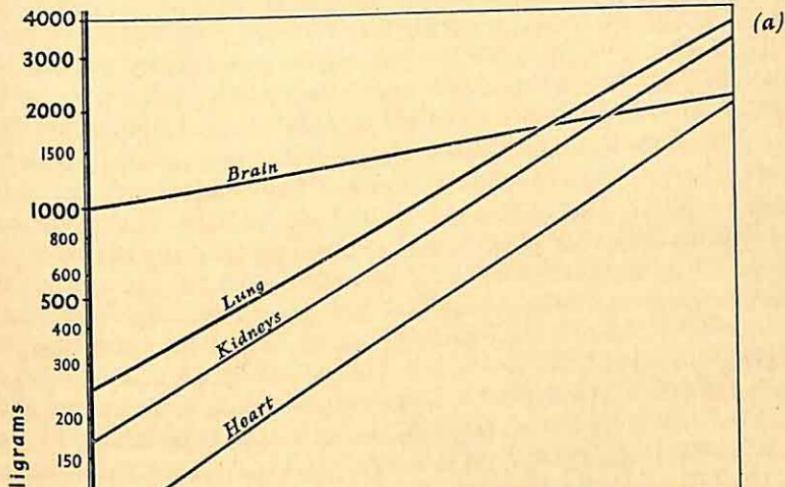
I think we all agree that we do not invariably find stages, but in certain phenomena we find more or less continuous curves, and in others, distinctive steps. The growth curve of body-weight in a fish, for example, is quite smooth and without inflection. In the rat, however, a lower mammal, the curve is in general similar, but a more detailed analysis reveals that it is actually composed of two 'growth cycles', the transition from the first to the second cycle being relatively sharp and corresponding to the beginning of puberty. Finally, in man there is a very apparent growth-cycle added to the basic curve at puberty—the increase in growth-rate that Tanner calls the adolescent spurt (see Vol. I, p. 36). I want to emphasize that such steps are real. In physiological phenomena in the rat, for example, you find continuity with respect to certain characteristics and with respect to others you have breaks. For example, make an allometric plot

FIG. 2

## RELATIVE GROWTH IN THE ALBINO RAT

## (A) SIMPLE ALLOMETRY. (B) CHANGES IN ALLOMETRY

The discontinuities in relative growth appear at a body weight of circa 100 g., i.e. the time of puberty. A similar break is found in the growth-in-time curve of the rat. The figures give only regression lines: complete data and statistical analysis in Bertalanffy and Pirozynski (1952, 1953); Racine (1953).



(Fig. 2), taking total weight as the abscissa and some physiological variable as the ordinate. For the allometric growth of the heart, for example, we get an allometric regression line without a break, and the same applies to the relative growth of the brain. However, in the relative growth of the liver, we find a break; similarly, in basal metabolic rate and other phenomena. But when and if breaks occur, they all take place approximately at the same period, namely, at a body-weight of about 100 g, that is, just preceding puberty.

LORENZ:

I want to say a few things about stages in the development of behaviour in birds. I could not disagree less with what Professor Piaget has said about general stages. A general stage occurs only where the development of different functions coincides, is synchronized. The probability of such coincidence is small and we have true synchronization in the behaviour patterns of animals only where an obvious selection pressure has evolved a synchronizing mechanism. We have done two studies recently on the development of behaviour in birds: one is Ilse Precht's still unpublished paper on the development of the gaping response, or the begging response in little birds, and the other is Helga Fischer's longitudinal studies in geese. Now, there are two events in a bird's life, hatching and fledging, at which it is very necessary to synchronize function. When a bird comes out of the egg it must start to breathe, it must start to eat, it must start to move in a very specific manner, and it must start at once to learn several things, to learn what not to eat, mainly. Thus you can say that the newly-hatched goose is a very well-defined general stage. The same applies to fledging, when the bird suddenly begins to fly.

These are the only general stages which I could find in a bird's life because in studying, as we have done now, the development of certain activities in geese, we find that, although there is a very strict succession of stages for every single one of the activities studied, nothing in the morphology or the behaviour of the bird varies as much as the relative speed of development of different activities. The unsynchronization, the variations of these speeds of development may give rise to certain syndromes which always occur if certain desynchronizations have taken place. To give you a very clear example of this: in lightly-domesticated geese—they must have some domestic blood to achieve this—the mother-child relationship, in other words the following-mother relationship, which is a clearly-defined type of behaviour, may survive abnormally long, and sexual activities may, in these semi-domestic animals, appear abnormally early. Then you get a syndrome which is the most beautiful Oedipus behaviour you can imagine: the bird insists violently on copulating with its mother

and with nobody else. If he has a father who is non-domesticated, whose sexual activity therefore arises later in the spring, the father doesn't object to this. We have a Canada domestic hybrid who is called Oedipus for this very reason, and I can guarantee to get the complex of Oedipus behaviour experimentally at will. I only have to take a wild Canada gander, and a domestic goose, and pair them, and have them raise their children and this behaviour appears regularly.

Now, Helga Fischer has shown how more subtle non-synchronization and occasional skipping of a stage results in a very interesting disturbance of pair-formation. For instance, the ganders are more active in certain pre-copulative sexual activities called distance courtship, which is the only behaviour pattern at this stage to show sexual dimorphism. It is this distance courtship which sorts out heterosexual pairs, and if it is skipped, which may happen for environmental reasons, then homosexual pair-formation occurs for the reason that the only other sexually dimorphic behaviour patterns appear much later in relation to real copulation. Thus again, de-synchronization and/or skipping of a stage results in highly specific disintegration of behaviour.

Thus I wanted to say how necessary it is to study the stages of *each* behaviour pattern, of each type of behaviour, thoroughly, and then look for co-ordinating mechanisms. We do find very definite co-ordinating mechanisms which may effect re-integration of this disrupted behaviour at a later stage.

You may be surprised at the difference of age at which the stages may occur. We have case-histories of distance courtship, the normal beginning of pair-formation, occurring at one year old in one bird, and at over five years old in perfectly normal others. A species with such a tremendous life-span as a greylag goose can afford to spend quite a few years on pair-formation; no very important selection pressure is brought to bear on the quick formation of pairs because a pair once formed survives for a very long time.

Ilse Prechtl has described some very interesting cases of overlap of function giving rise to non-adaptive behaviour. There is a mechanism which orients the gaping of the young bird upward as long as it is blind, and a later-arising mechanism which visually directs the bird's gaping towards the parent; and there is a time when both are active with the result that the bird gapes in the resultant of the two orientation responses. In cave-nesting birds the first gaping response is released by auditory functions, and this releasing mechanism is switched off from one hour to the next at the time this bird begins to gape visually. This switching off of the auditory releasing mechanism is reversible because if you stick a little black paper on the bird's eyes it begins to gape on acoustic

stimulation again. So this shows definitely that there must be a survival value for the bird, not only in each of the two things, but also in a definite mechanism for making a jump where originally there was an overlap.

I mention this to show that synchronization, in other words a general stage, is a secondary effect due to a particular mechanism necessary to effect it. In the development of animals, there is a definite survival value in *not* synchronizing the development of different functions and/or structures because if you change too much at once there is danger of disintegration. The instability of transitional stages couldn't be emphasized more dramatically than by the animals dying. Anybody who has bred cichlids knows that the brood perish either immediately after hatching, or at the point of metamorphosis from the non-swimming, beak-fed stage, to the swimming stage. Those are the two metamorphoses in a cichlid's life. In a bird, anybody who has bred ducks and geese knows that there are also two points of danger—one is after hatching and the other is at fledging. These are the two points which, like the disintegration of human behaviour in puberty, are stages where necessarily many things must be changed at once. Otto Koenig in Vienna did a very beautiful study with the child-psychologist, Sylvia Klimpfinger, on the ontogenetic development of behaviour in hounds; and they showed that the mortality of young hounds was greatest at the phase of accelerated transition.

I was going to say exactly the same thing Grey Walter said about thresholds, that of course overt behaviour very often begins quite suddenly from one hour to the next when the threshold is reached. It varies from case to case, however: some instinctive activities begin slowly and get more and more intense from day to day, whereas with others the sudden crossing of a threshold makes an apparently huge jump; either may happen.

#### PIAGET:

I must now go on to give a definition of affectivity as Lorenz, Bowlby and others have requested. French-speaking psychologists speak frequently of affectivity in contrast to cognitive functions, but a word with such significance does not really exist in the English language. Consequently, we must come to some agreement here about the implications of this distinction: is it only relative to a certain cultural language or does it correspond to something real in psychological fact?

I would mention first three authors whose psychological work is very different, but who have each dealt with this distinction. I am thinking of Kurt Lewin, Pierre Janet and Claparède. Kurt Lewin, as you know, introduced the idea of 'Gestalt' into the study of social

and affective psychology and for him the problem was one of characterizing affectivity from the theoretical approach. Lewin proposed extending the original notion of 'field' in Köhler's language to what he called the 'total field', which would include the ego of the subject with his reactions to objects and not only what is perceived or conceived in the external world by the subject. In the total field Lewin introduced a distinction between the *dynamics* of the field and the *structure* of the field. These are two characteristics which are indissociable because there is never structure without dynamism or dynamism without structure, but they are never reducible one to the other. In Lewin's language affectivity is the dynamics of the field and the cognitive functions are the structuration of the field. This seems to me a relatively clear criterion.

Pierre Janet distinguished in behaviour two sorts of action which are also very different but are always found at the same time: what he called primary action and what he called secondary action. Primary action is the relation between the subject and the objects; for example, the action of going in the direction of any place in space, or of manipulating objects. Secondary action, on the contrary, is the energetic—or, as Janet calls it, the economic—regulation of the primary action. These regulations consist of regulations of activation: activation in the positive sense like effort, pressure, etc., or activation in the negative sense, like fatigue or depression, which slow down action. On the other hand, there are regulations of termination and not only of activation, some of them positive (that would be joy or the feeling of success) and the others negative (which would be sadness or the feeling of failure). In all elementary behaviour Janet thus always finds the primary action which corresponds to what Lewin calls the structure of the field and then the secondary action which corresponds to Lewin's dynamics of the field.

Claparède tells us that in all behaviour there is an aim pursued and means used to attain it. On the one hand we have the value of the aim; on the other hand we have the method for attaining this aim. The value of the aim is affectivity, the technique is intelligence (in other words the cognitive functions of perception, sensorimotor co-ordination, intelligence, etc.).

Thus in three different authors I find the same dichotomy of two indissociable and complementary aspects of action, but two aspects which are irreducible. Agreeing with this—I would say that affectivity is the regulation of values, everything which gives a value to the aim, everything which releases interest, effort, etc., and then I would say that cognitive functions are the total of structural regulations.

I will take two examples: one very elementary example from the point of view of the level of development—that of instinctive conduct. In French we have no word to designate the sum of instinctive

behaviour, we just say 'instinct'. In German there are two words which correspond precisely to the dichotomy I was speaking of just now. In German one says 'Trieb' for the valuation of the aim and the pressure which directs towards the aim, and 'Instinkt' for the technique, that is to say for the cognitive functions. This is exactly the dichotomy I was thinking of for affectivity and the cognitive functions. At the other end of the scale I will take a higher behaviour pattern such as mathematical reasoning. Mathematical reasoning is highly cognitive of course; it is a combination of symbols, operations, etc. This is the structure of the field. Mathematical reasoning also necessitates, however, interest and effort; there can be fatigue, there can be reinforcements (will-power, etc.) and then there are sentiments such as the aesthetic (every mathematician will say, 'An elegant demonstration', or, 'A clumsy demonstration'). All this corresponds to an energetic regulation of the action, it is affectivity.

I think, then, there are two dimensions to every behaviour pattern, whatever it may be, and that it is legitimate to use a general word to designate the two terms of this dichotomy.

#### LORENZ:

I would like to call attention here to the very old paper of Wallace Craig who died last year—*Appetites and Aversion as Constituents of Instincts*, in which he gave exactly these two conceptions and in an identical way. It is a very interesting symptom of the effects of my participation in this group that I now miss badly at this table an American psychologist, a good behaviourist, a good learning man, because I would like to put to him just one question: whether a learning theorist would be content with my definition of affectivity which I think is more or less identical with Piaget's.

I would say what we call affectivity is the subjective side of all processes which effect positive or negative conditioning—which is the objectivistic behaviouristic way of saying goals and aims. I have been re-reading McDougall lately and I have always been struck by the ease with which he can be translated into pure objectivistic terms, if only he wouldn't insist on being a vitalist! Now I am sure that very many of my American friends on hearing Piaget's exposition of affectivity would accuse him of being a vitalist, just as he accused me! Still, a short time ago I was a vitalist. I want to translate what Piaget has said in such a way that the wisest Watsonian couldn't object to it. Now in even the purest kind of cognitive behaviour you still have to have an aim, you have to have an emotional signal flag as the goal for which you are striving, and I think that you could get the behaviourists to agree by saying that this is reinforcement. Everything that is a gain is a reward and every reward is by definition

something which is a reinforcement. The reward is in our subjective experience but this still constitutes reinforcement. Everything that we experience as punishment constitutes the opposite. And that's all in Craig.

This affective aim, the rewarding flag at the end is, of course, characteristic of all behaviour and McDougall is quite right that this aim is still there even if the animal doesn't move. It is there in what Monica Holzäpfel so aptly termed the appetitive behaviour directed at quiescence. This does not mean the animal is going to sleep, but signifies its appetence for the state of rest from disturbing stimuli which causes all the responses by which an animal is driven into its optimal habitat. Running down a scale of humidity or up a scale of temperatures, are responses which Monica Holzäpfel has termed 'Appetenz nach höheren Zuständen'. And this is what in Wallace Craig's terminology would be called aversion. In Wallace Craig's terminology everything ending in the performance of an instinctive act is an appetite. And everything that acts in quiescence is aversion. The psychology is doubtful because, while it is true that in most cases where you strive at a consummatory act like eating or copulation, it is for pleasure, the converse is not true. When you run away from cold or you run up a temperature scale there is pain on one hand but there is still pleasure on the other; it's punishment *and* reward. So in the case of appetite it is true that the thing works on the reward principle but it is not true that the Appetenz nach höheren Zuständen works exclusively on the punishment principle, as Wallace Craig thought.

#### MONNIER:

I should like to bring to the definition of stages of development in the child, some facts concerning EEG changes with age.

A qualitative analysis of the EEG development in three boys of the same family (6 to 11, 12 to 15, and 13 to 15½ years) in a longitudinal series shows that the development of cerebral activity occurs continuously and progressively, according to a structural plan which is expressed partially in the record of each subject at each age, but completely only in early adulthood. Thus the 15-year-old adolescent shows a definitely organized pattern, towards which the still imperfect structures of previous periods were tending.

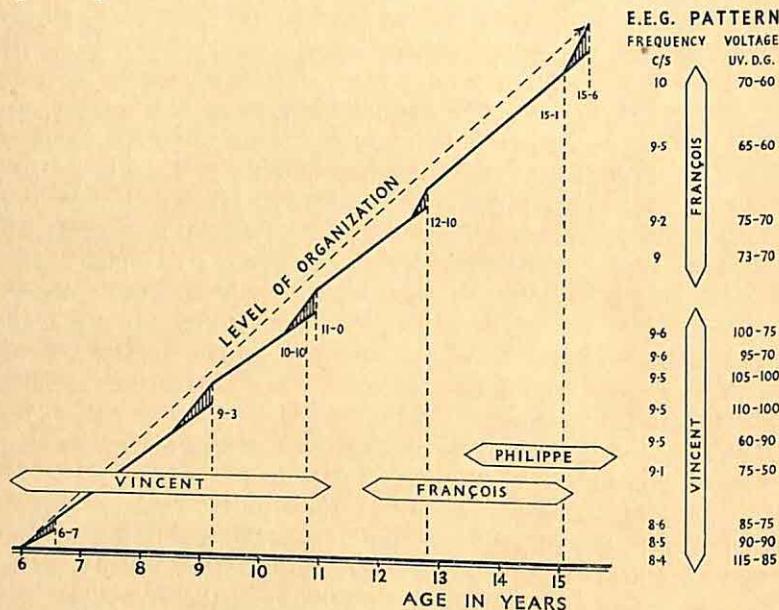
The characteristics of complete development illustrated by the electrographic pattern of the 15-year-old boy are: progressive differentiation, systematization and localization of the cerebral electrical activities. These features are particularly true as concerns the alpha rhythm, whose polryrhythmic components diminish in favour of a few dominant components; thus the alpha rhythm becomes more monorhythmic and more monomorphic. These

dominant components increase in frequency, abundance, and regularity in the posterior part of the brain, with a clear tendency to a more precise location in the occipital region and with predominance on one side. Moreover, the alpha rhythm becomes better organized into harmonious spindles. Parallel to the organization of the alpha rhythm in the posterior region, the activity of the temporal region

FIG. 3

DEVELOPMENT OF ELECTRICAL ACTIVITY OF THE BRAIN  
IN ONE CHILD (VINCENT) FROM 6 TO 11 YEARS AND  
ANOTHER (FRANÇOIS) FROM 12 TO 15 YEARS

The cross-hatched zones indicate the times of apparent discontinuity or stages. On the right are frequency and voltage of alpha rhythm.



becomes also less polrythmic and polymorphic; the small components of the delta band diminish in abundance and voltage, and the theta-rhythm frequency band becomes narrower. Finally, the theta components also decrease in voltage; their traces interfere with traces of rapid beta components. This theta rhythm more or less entirely disappears in resting conditions towards adolescence, between 12 and 15 years. We may find it later, of course, under emotional conditions.

Many data suggest however the existence of stages within the continuous development. Fig. 3 shows on one axis the pattern of

organization, and on the other axis the age in years. The pattern has been analysed qualitatively and to some extent quantitatively in frequency and voltage. The average frequency increases from 8.4 to 9.5 cycles/sec. in one boy. In the two older boys, the frequency also increases, from 9 to 10 cycles/sec.

Within the continuous development of the brain rhythms, one can distinguish certain epochs at which the organization of the electrographic pattern becomes better defined, structured, and consolidated. These epochs, which produce the impression of a certain discontinuity in development, perhaps more apparent than real, can be called stages. Thus, the youngest child reaches a first stage of development at 6.7 and 7 years. Then a second one at 9.3. This first step is very typical. The predominance of the alpha rhythm on the right side becomes irreversible. Once this last stage has been reached, further examinations carried out during the tenth year do not show abrupt alterations any more; however, at 10-10 new improvements allow one to foresee a next step towards 11 years. Consequently, we may postulate the existence of three stages of development in this child, at 6.7 to 7, 9.3 and about 11 years. As concerns the stages of development during the prepubertal and pubertal period, we have got some data from the shorter series of examinations carried out in this boy's brothers. At 12.10 one boy's EEG develops some more homogeneous and less polyrhythmic pattern. Finally, in both older boys, the pattern of adolescent cerebral activity is achieved towards the fifteenth year. If we compare the rate of development of the cerebral activity in two subjects during a similar period (12-15 years), we are able to distinguish individual differences. The EEG shows certainly a higher degree of organization in one boy at 15 years than in his brother.

My conclusion will be very brief. If we compare the characteristics of the stages defined by Piaget and Inhelder on the basis of their psychological investigations with the characteristics of our EEG stages, we recognise some correspondences. We detect a constant order of succession in the transformation of the EEG with the age. We have noted at one stage in the EEG signs of preparation for the following stage. Furthermore, we observed signs of growing equilibrium in the form of more and more monomorphic, localized, systematized and structured rhythmic activity with definite irreversibility. Once a pattern has been acquired, it will not regress to the previous stage, unless disease occurs.

## FOURTH DISCUSSION

### Psychosexual Stages in Child Development

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ERIKSON:

This meeting I am going to present a restatement of the 'Freudian' phases of early infantile psychosexuality. At the last meeting (Vol. III) I discussed *psychosocial* development in general and the development of the sense of identity in particular; and my desire to do so then sprang from my feeling that if one really wanted to speak of *human* ethology it was necessary to take into account the whole span of the first twenty years. It seemed necessary for somebody to point out that we cannot forever continue to compare the *small* human child with the *young* animal and think that the two will have parallels which will explain the principles of human ethology. Human ethology, rather, must encompass the whole, ever-widening circle of the pre-adult human's mutuality and fittedness: in relation to the maternal person, the parental persons, the play- and schoolmates, the training and the teaching adults, and the institutions which represent the ideology of the whole group. That all of this taken together is as important to a person when he is in his late teens as his mother was to him when he was at the earlier age is what I tried last time to illustrate with the history of a breakdown at the threshold of young adulthood.

But now I am grateful for the opportunity in the eleventh hour of this group's work to come back to the early Freudian stages. When the members of this Group speak of the 'Freudian stages' they mean, of course, the psychosexual stages of orality, anality and genitality; and it is entirely true that what Freud had first in mind was a reconstruction of early sexuality, that is psychosexuality, on the basis of psychopathological material. It is up to each of us to decide how much of Freud's original reconstruction in the psychosexual area he wants to accept—how much of it he thinks he recognizes in his patients' neurotic regressions, dreams, character deformations, or in the direct observation of children. But it has seemed to me now for 20 years that these stages have an importance for child development and psychosocial development in general which is mostly overlooked—by my psychoanalytic colleagues as well as by those in other fields.

'Oral', 'anal' and 'genital' are terms which refer to body zones: meaning zones around the mouth and the upper nutritional tract, the eliminative organs, and the genital organs. And it was Freud's theory that a certain amount of libido or sexual energy is attached to these zones during successive stages of development; that the child has experience of a highly libidinal, emotional, affective kind centring in and around these zones; that societies are more or less prohibitive in regard to the employment of these zones in infantile habits, and that the human being suffers from society's interference with the naïve, sensual, sexual experiences attached to the zones. What Margaret Mead said about the cognitive stages as developed by Piaget and Inhelder holds true for the psychosexual stages as well: they are part of 'a process of maturation meeting a process of education'—education not only in the sense of prohibition or guidance, but also in the sense of the transmission of the particular culture's version of reality. In order to see this, however, one must add to the concept of the psychosexual *zones*, that of 'organ *modes*'—modes meaning ways of 'going at' something—modes of aggression, if you wish, in the original Latin sense of *adgredere*. The appearance of a new stage is, most generally, the appearance of a new dominant mode of going at things, by way of a newly-matured executive part of the organism. In this sense then I would postulate that during those stages described by Freud as 'psychosexual' a number of organ modes become the highly affective modes of going at things, and that at the same time, these modes become the concern of child training.

'Incorporation' would be the dominant organ mode for the zone around the mouth, the oral zone. It represents a general attitude of 'accepting' experience of a kind that is *offered* rather than the kinds that one must know how to take or to reach out for. One may say, however, that such incorporation at the beginning of life is also the dominant mode for a number of other zones, such as the eyes, the ears, the senses in general, and the whole tactile surface. Therefore I would speak not of an oral stage but of an oral-sensory-tactile stage. Nevertheless, the mouth, as the organ of nutritional and libidinal intercourse with the nipple and other sources of food, remains the centre of this organization of incorporative organs.

To the dominant first mode there is soon added a dominant second mode. The second incorporative mode would be incorporation by way of *taking*—more active incorporation through a more co-ordinated reaching out and seeking out: biting, grasping, focusing, etc.

As these get established libidinal attention as well as growth-energy shifts to another zone-organization: retention (*retenere*) and elimination (*ex* and *limen*) are the modes dramatized in the sphincters

and the muscle system, this occurring during the second year. There is a certain innate 'ambivalence' in the organ systems which have to alternate and synchronize both retention and elimination, which is what the sphincters have to learn. It is obvious in the motion of alternate opening and closing that the organism becomes ready at this time alternately to hold in and to let go, to grasp and to throw away, to hold on and to repudiate in a number of organs and in a variety of social interactions.

Later, development as well as libidinisation shifts to locomotor and infantile-genital organs: intrusion, in the Latin sense of *intrudere*—'thrust into'—is the organ mode connected with psychosexual sensations in the phallic-urethral zone, in the third and fourth year. Inclusion becomes the dominant mode for the female genital organs. Both come from a common matrix which boys and girls share, even as they share a common maturation in locomotor development and locomotor passion, as brought about by individual idiosyncrasy or cultural provocation.

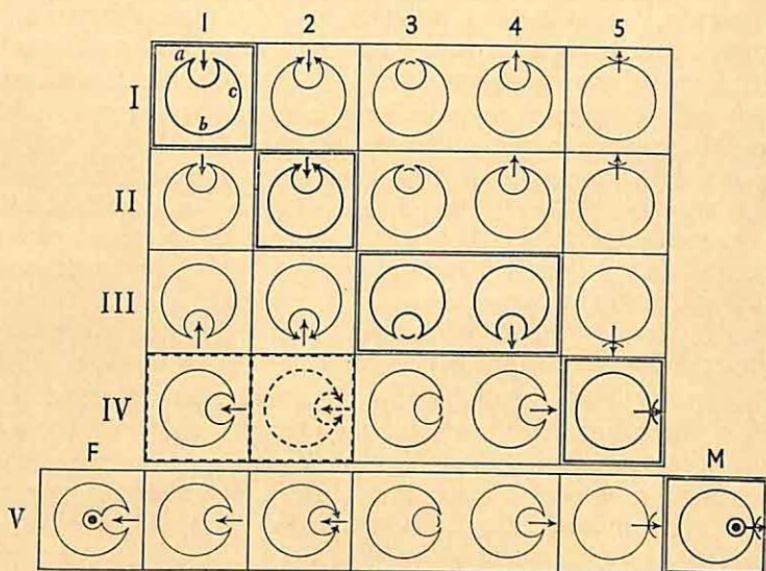
Each mode, then, is originally 'attached' to a focal zone (Freud's erotogenic zones): (1) mouth and throat; (2) rectum and urethra; (3) genitals; and a peripheral contact system: (1) sensory and tactile; (2) muscular; and (3) locomotor.

I really don't feel able to discuss in brevity the question as to whether these mode-maturations are the function of 'instinct' or not. There is something quantitative about them, an amount of urge which must find its action, which Freud has called *Trieb*. To Freud this was primarily an energy concept—a fact which is often forgotten for semantic reasons: in English the German *Trieb* becomes 'instinct' whereas *Trieb* should really be 'drive'. I make this matter easier for myself by speaking of *instinctual energy* and *instinctive patterns*, i.e., of instinctual forces which endow instinctive patterns of contact-seeking with energy. It is one of the main aspects of human life that in man's long childhood instinctive patterns are more variable and instinctual energies more displaceable than they are in the animal. I think we would save much misunderstanding if we would always assume that Freud is speaking of a quantity of instinctual energy temporarily or permanently 'attached' to a particular organ system and its particular patterns of behaviour. These instinctual energies are obviously related to Professor Piaget's 'affective' energies. On the other hand, there are also instinctive patterns (such as the organ-modes) which are related to the maturation of the cognitive systems; so the problem remains how to study the overlapping of the energy aspects and the pattern aspects of that long period of maturing, playing, and learning called childhood.

Let me now discuss the probability that in the existence of organ-modes and in their relations to each other we have a set of pheno-

mena akin to what Konrad Lorenz referred to as 'relatively autonomous processes'—processes which stubbornly seek an equilibrium within themselves; but which also, just because they are so stubborn, create critical periods for the human being in that they refuse to be harmonized with the various reaction velocities of other systems. This in fact is what I think Freud first saw in his psychopathological investigations. Let us first look at a figure (Fig. 4) which tries to indicate the inner relation of the organ modes to each other.

FIG. 4  
INNER RELATION OF ORGAN MODES



This diagram is a systematization growing out of the observations of children's symptoms and children's play, and out of the comparison of child training patterns in various cultures. I.1 is a sign for the incorporative mode. II.2 is the sign for the second incorporative mode of clamping down, grasping, biting on, taking. III.3 is the retentive mode, meaning closing for the sake of holding in or keeping out. III.4 represents the eliminative mode, IV.5 the intrusive mode, and IV.1 and 2 the inclusive mode.

MEAD:

You ought to add that this chart is really set up for males only.

ERIKSON:

Yes. The letter *a* represents the oral zone, *b* the anal/urethral zone, *c* the genital zone. As you see, *all the modes, and not only the dominant*

one, 'exist' at the same time (horizontally) in all zones, and can, in fact, become relatively dominant—for pathological, idiosyncratic or cultural reasons. This is one rule this chart demonstrates. However, if any of these secondary modes is over- or under-emphasized, then there is an interference with the supposedly dominant mode and with the next mode which can either be called into life too early or too intensively. Inappropriate clamping down (I.2) for example, can happen if the baby is either inclined or forced to close up. And there can be also an intrusive pattern (I.5) of—as it were—screwing himself on the breast. But the dominant mode at the first stage, the mode that must be mature enough to meet the particular system of child training which in turn must be ready to meet the mode, is incorporation, I.1. Thus, any early and strong deviation, modification or variation will show up systematically in the whole organization of modes—and to trace such variations in psychopathology and in anthropology was the second purpose of the chart.

Progression by growth and differentiation proceeds along the diagonal (as it did in the psychosocial stages shown in Vol. III, p. 168). This is the epigenetic principle: all of the modes are present in all organs from the beginning, but each organ has a dominant mode which has its developmental time.

LORENZ:

What strikes me quite particularly is that the stages of first grasping only and then grasping and letting go alternately, are very well marked in locomotion. In practically all vertebrates who walk on feet you get first a period when the animal is very well able to grasp but is not able to let go. He just hangs on, which is of survival value of course for not falling out of the nest. I think that these two stages of grasping—first grasping and then retention and elimination alternately—are highly characteristic of the ontogeny of locomotion in all vertebrates that are not born in stage 3.

KRAPF:

I would mention the very early appearance of the grasping reflex in the human neonate, a grasping in which the infant also cannot let go. It already appears in the foetus, and is undoubtedly one of his first manifestations of motor behaviour.

ERIKSON:

I would be intensely interested in a systematic comparison between these matters of human development and phylogenetic material; and I hope that sometime we can look at all of this in an ethological way. But I must emphasize that I am not now talking about maturing capacities. I am talking about something which Professor Piaget

calls affective, and others call emotional—that particular investment in subjective experience which Freud calls libidinal; that ambitious, stubborn, and excited interest, in going at things in certain ways, of completing an act, coupled with the high degree of frustration from not being permitted to complete it or not being able to complete it.

I must emphasize this, because in most of the discussions of this group this central problem has remained peripheral.

LORENZ:

I entirely agree with this and I hadn't misunderstood you, but all you said now about affectivity applies exactly to the affectivity of motor patterns. I was thinking about the development of monkeys which Katerina Heinault has particularly studied. All this theory about frustration applies exactly to everything a little baboon does, not with his mouth or his genitalia, but quite exactly to what he does with his hands and feet. *Quite* exactly! I emphasize that this is not an objection, it's an illustration.

ERIKSON:

A very important illustration. What I want to get to next is a reformulation of these stages as a series of *encounters* such as only human life institutionalizes. Each stage permits an intensive encounter with particular educative measures. Cultures have, of course, every interest in fully meeting the child's maturational readiness in these respects, and they have done so, one after the other, with highly emotional and magical measures of training. This the chart cannot illustrate. It only indicates a maturational sequence of 'readinesses', and like all maturational diagrams (only more importantly) it fails to take into account such elements as intensity and duration, which are widely influenced by the environment.

MEAD:

Don't you think too that the chart was highly influenced by being made up in America at a time, in the early 1930s, when nobody could think about anything but zones, when books on children would have sometimes a hundred pages on toilet training and two on talking, and the culture was treating the child as a set of zones on the trunk of the body. Writers on child care ignored arms and legs, ignored speech, ignored cognitive development. When I first saw the chart I found I could put half my cultures in these terms because they also were obsessed by zones and trunks, but I could do nothing at all at first with a culture where the emphasis was on the whole body and a great deal of importance was given to the locomotor and tactile systems. You now include these in your statement.

Well, the chart depicts only the organ modes centred in the erogenetic zones which (for good biological reasons) *are* on the trunk. In my book (Erikson, 1950) muscles and limbs are already acknowledged in so far as they are concerned in this particular problem of organ modes. But I'm glad Margaret Mead referred to the problem of transitory one-sidedness in psychoanalytic concept formation; it occurred to me that I should say something about this. Psychoanalysis develops in a highly dialectical way. Somebody said to Freud once: 'Five years ago you said exactly the opposite', and Freud said 'That was right then'. In a dialectical way Freud always wrote as much against as for something, and many of his statements become clear only in the context of the then necessary antithesis. Take, for example, the question as to whether a memory reported today is more representative of the situation in which it emerges, or of the past situation of which it claims to be a remnant. In the debate between Dr. Bowlby and Professor Piaget at the last meeting (Vol. III, pp. 156, 160) Bowlby quoted Freud as saying that 'memories are indestructible'. Actually, Freud said that 'early structures' (and that is more than isolated memories) are 'never quite destroyed'. Now that was written in a general climate in which it was assumed that the child was not affectively aware of many of the most important things that were happening to him, did not care to be aware, and did not, later on, remember. Freud concluded from later disturbances that such 'silent' events had, in fact, been deeply experienced and very deeply registered. To my mind this does not mean that memories as such are indestructible; Freud after all was the one who described what happens to early experiences, how they are disguised, transferred, displaced, repressed—he described the laws of their destruction while maintaining that some of their energy and essential imagery persisted. At any rate, the point I want to make is: when you quote Freud, you must say what he wrote *against* at the time.

Every step in psychoanalytic theory has the most far-reaching implications, for the philosophy of life, for the treatment of children, for one's attitude toward one's own neurosis, and so on. It is always significant on that frontier of living where we touch the irrational, the 'inhuman', the ugly, the frightening. So in response to every change in emphasis (it is happening now to my concept of identity) a hasty kind of pseudo-integration takes place, a pseudo-synthesis of everything that is known up to then, a totalization, you may say, of partial knowledge. In psychology, this reaches into the ethical and philosophical much more quickly and deeply than do similar pseudo-integrations in other scientific fields. In order to give security to their children and to their patients, people have to offer premature closures of knowledge which, though the best one can have at a given time,

may still be conceptually spurious. It is for this reason that in psychoanalysis as a 'movement' we have had any number of Utopias which have had to be undone or left by the wayside before the next step could be made.

FREMONT-SMITH:

This is not true only of psychoanalysis. I think almost every scientific development has its eras of Utopia, and its needs of undoing. In fact, one of the things that this Group has come together for is for a bit of mutual undoing!

LORENZ:

The most beautiful example of this relative truth is behaviourism and purposive psychology. If you don't know whom McDougall is arguing against, you will think that this great genius is a perfect fool; and exactly the same is true of behaviourism. Behaviourism and purposive psychology give a wonderful example of Hegel's doctrine that the thesis is only true with its antithesis.

ERIKSON:

Coming back to Fig. 4—in the first three stages we assumed a mode development independent of sexual differences. This is unlikely, but a chart can do only so much; and, as a chart, it does presuppose that the later stages are present in the earlier ones. In IV.5, then, the intrusive mode dominates the others in the boy's life. Yet, since there is a high degree of bisexuality, the 'feminine' mode of inclusion, a kind of 'enveloping' attitude (IV 1.2), is secondarily emphasized in the boy. In the girl this mode is dominant, while the intrusive mode becomes secondary. Incidentally, this arrangement makes oral incorporation (I.1) a precursor of genital inclusion (IV 1.2). This gives the girl's whole chart a different slant of great significance.

The further development of these modes, in so far as they remain sex-bound, is indicated with the little sign of a circle with a dot inside (V). It represents the fact and, if you wish, the instinctive knowledge of the fact, that deep inside the body procreative modes are waiting. Their physiological focus is obviously in the boy the *Anlage* of the semen and in the girl that of the ovum. This something inside the body gives all these modes a particular meaning, lending libidinal energy to the whole complex of wanting to be a father, of playing with bows and arrows or guns and automobiles, or whatever the equivalent technological symbol is to express both intrusion and locomotion. In the modes of inclusion, there is also an increasing orientation towards a future role of incorporating for the sake of developing something inside. This again was at one time a rather important thing to say because psychoanalysts tended to consider

pregenitality a mere preparation for exercising potency, and for learning to join in the genital climax, omitting the further development of the libido to what I call generativity. This then is represented by the addition of an inner centre to all the psychosexual modes.

Now before I come to the main point, I would like to indicate briefly where this chart overlaps with the chart on psychosocial development which I presented at the third meeting (Vol. III, p. 168). You will remember that the problem of Basic Trust vs. Basic Mistrust was the first psychosocial problem, occurring in the first year of life. This would correspond in the present chart to the problems of incorporation. Basic trust is the lasting capacity to overcome basic mistrust in spite of everything that life may bring. The child acquires the basis for such faith, as Bowlby so dramatically shows again and again, in the at first 'incorporative' meeting with the giving maternal figure in the first year. The second psychosocial problem, autonomy vs. doubt and shame, develops with and out of the interplay of retaining and releasing, which culminates in a certain pushing away of the mother while yet holding on to her. It needs basic trust to develop autonomy, while shame and doubt at that age would make the child want to go back and check up on mother (and her reality and morality). This coincides with the second stage, that of retention and elimination. All this is dramatically illustrated in psychopathology, and popularly in what the Germans call *Trotzperiode*. Finally the psychosocial alternative of initiative and guilt is related to the stage of intrusive-inclusive differentiation: for it is out of this third stage that children have to retain the capacity to develop the uninhibited ability to exercise male and female initiative according to the ideals of their culture.

The point I want to make now is that Freud, by finding in his psychopathological work the greatest relevance of the connexions which I briefly illustrated in the diagram, really pointed to more than he cared to elaborate at the time when he was exclusively interested in the importance of this system for the development of healthy or morbid psychosexuality. The early encounters of these maturing organ modes with the specific educative processes, which are meant to meet them half way, establishes, I think, a basic grammar of what I call *social modalities*. For example, in order to get something in the incorporative way you have to have somebody give something—and the general setting for the act of giving and receiving will, in large or small details, vary from couple to couple and from culture to culture. In the interplay of getting and giving, then, a certain basic style materializes for the experience of getting and being given on the part of the child and, on the part of the maternal person, for giving to somebody who needs to be given to. The satiation of the recipient, his signs of thankful gratification, and the satisfaction of the giver,

and her feeling of having done a good job in the eyes of her society, complete a cycle comparable to those described in animal ethology. But human cultures vary in their ways of giving things to the baby and of withholding things from it and of justifying their particular way of doing so. In any given culture, therefore, a child experiences the modes in a special form. Here, an important thing has to be added; good and evil permeate human life to such an extent that each of these modes can take on a highly negative and a highly positive connotation: human health as well as virtue depend on their balance. To get by being given can, on the one hand, become an excessively demanding attitude in life, and it can also become the modality of receiving with good grace. Any number of variations are possible between those two.

As we think of such combinations it is good to become aware of the language in which we express them. There is a relationship, I think, in any language, between certain very basic terms and the basic social modalities. One cannot help noticing to what extent the whole culture, in its technology and in its personal relations, expresses itself already in those early encounters. To take (that is, the second mode) can mean to acquire, to secure, to learn to grasp; but also to snatch, to rob, to take away from someone. Then 'to hold on' can become to hold in, to hold away from, not to give up; on the other hand it can have the positive connotation of holding, of maintaining. Especially is the expression 'to have and to hold' a very nice one in English. It means you hold on, but for the purpose of taking care of what is held. Maintaining can be stubborn and negative, or positive, as in maintaining interest and sustaining an activity. Then 'eliminative' would be in English 'to let go', which can mean to let, to let be, or to let pass. On the other hand it can mean 'to eject', to let loose destructively. Finally, the intrusive and inclusive modes are best expressed in the term 'to make' as a form of 'to conquer' a person. One speaks of 'making a girl' which means to take her, to intrude upon her; one also can 'make a job', meaning you did it, you managed it. Intrusion and inclusion are forms of 'making people'. You can make them by intruding into them, or by snaring and attracting them. Genitality in its generative aspect is later contained in the nice English word 'to beget' which means to intrude or include in order to create a child. This of course is the more mature, more mutual form of making something and making something with somebody. This comes late in the human, who has to learn first to make things in the sense of finishing them, of doing them with somebody in some division of labour—which is where the school age begins. The social modalities, I would mention in conclusion, develop out of an interplay between the ability cognitively to complete certain operations in regard to the physical world, affectively to

interact with a growing radius of relevant people and things, and ultimate to integrate and maintain both.

If there were time to present a case-history I think I could show more convincingly how organization of organ modes tends towards an equilibrium of its own. This could be seen in comparing a child's social behaviour with his play behaviour, his phantasies and his habits. Such an equilibrium could also be shown in a systematic comparison of all the permissive, provocative, and punitive trends dealing with the zones and modes in a homogeneous child training system. Freud, of course, saw mainly the punitive in child training systems—which he viewed as mostly arbitrary or hypocritical or at any rate senseless—an idea which easily suggests itself if you consider only the victims of a system. However, if you look at the various cultural systems you find in each at least an 'instinctive' rationale, which is the best we have until we can develop a universal, rational child training system. I have tried to show this in comparing two American Indian tribes (Erikson, 1950).

When Bärbel Inhelder is testing a child, that child is supposed, for example, to match cards depicting two containers of water. But he may for a moment become more interested in seeing the water run out of the upper container into the lower container than in concentrating on the cognitive problem. In other words anyone motivated to concentrate on the cognitive has to suppress the mere enjoyment of making the thing function and the playful wish to match things aesthetically. Professor Piaget told us yesterday something about the equilibrium of the cognitive function. But as this is being established there must be some more general tendency in the child which permits him to concentrate on that which will lead to a cognitive equilibrium and to exclude other aspects of the mind which are seeking *their* kind of equilibrium: the psychosexual, the psychosocial, etc. This equilibrator behind all equilibrated functions is, I think, what we call the ego in psychoanalysis. This important aspect of psychoanalytic theory has been ignored in the precirculated essays in Part I.

There has been in these essays and in the discussion an exclusive emphasis, it seems to me, on the instinctual and energetic side in psychoanalysis. What has been completely ignored is the long development of structural concepts combined in what is called 'ego psychology'. Freud, back in 1920, in *The Ego and the Id* called the ego 'a cohesive organization of mental processes', i.e., an agency for a central equilibrium (the equilibrium of the child and not of its part-functions). In psychoanalytic ego psychology there is a whole literature on 'inner structure', on the development of a cohesive organization of mental processes which tends to create or to restore the whole child's equilibrium when he finds himself in a situation

where one function is to be concentrated on and others to be put aside.

In regard to the term ego, of course, one again has to overcome very great semantic difficulties. As far as the variety of its implications in different languages is concerned, the term 'substance' has nothing on the term 'ego'! When we say in America 'that woman has an ego!' we mean vanity, egotism. The German 'Ich', especially in its heavy existential implications, means something very different, and so does the 'moi' of *'L'état c'est moi'*. Nor is the psychoanalytic ego the 'self' as it appears, for example, in a Jewish story. An old Jew comes to his physician and says, 'Doctor, my feet hurt me, my stomach aches and my head is heavy. And you know, doctor, I myself don't feel so good either.' That is the self but it isn't the ego.

Brain physiologists before Freud called ego an area in the brain which is coherent and maintains its coherence, its equilibrium. Such equilibrium theories seem to be related to early economic theories. At any rate, from the beginning Freud paid attention to the problem of inner structure, to the balancing machine, as it were, for which his energies are the fuel: only that the human organism is a growing machine in which new structure forms through growth and through experience. Many aspects of what has been referred to here as 'organizing thresholds' and 'step functions', or as the reorganization of the total mental condition at times of 'multiple breaks', were anticipated by Freud a good forty years ago, while only his instinctual theories became popular, even among scientists. One must grant, however, that Freud's papers are always concerned with a more mystical central function rather than with observable part-functions, and they have a highly speculative character. Nevertheless a conceptual convergence clearly exists between Piaget's and Freud's ideas of mental structure.

Let me, in conclusion, repeat the obvious. When we speak of the human child, the distance from hatching to fledging, from birth to maturity, is tremendously longer, tremendously more complicated, than in other animals, and also tremendously dependent on variations in the environment. Therefore, it is necessary to study all the stages of instinctive, cognitive, symbolic and social equilibration that are necessary to build up to the time when human fledging can occur successfully—that is, at the end of adolescence. I understand very well and I certainly agree with John Bowlby that from an ethological point of view it is important to investigate early biological mechanisms such as those which permit a child to stand separation from the mother. I personally feel, however, that from the beginning to the end of childhood, society plays a decisive role. At the end, as I tried to illustrate at our third meeting, society must verify with ideology, and with opportunities, what the child has learned from the

long dependence on his parents. At the beginning, some kind of organized faith must provide the mother with a periodic restoration of that basic trust through which she can become trustworthy—so that trust may be created in the baby.

INHELDER:

It seems to me very important to remember, as Eric emphasized, that one should study not merely the development of the ego but also how the social environment of the individual stresses certain norms of behaviour. We must, of course, seek not only internal laws of equilibrium concerning the individual as such but also laws of equilibrium regulating interactions between the individual and his social environment. Thus, in our society, social norms which played a part in education became more flexible during the last decades, partly owing to the influence of psycho-analytic ideas. Nevertheless I see a certain danger in the superficial extension of these ideas, since they also impose new norms of behaviour which become more or less obligatory, at least for certain individuals. My own opinion is that the individual contributes very largely to his own equilibration and is not merely in some way the inevitable victim of either an internal development occurring *sui generis* or of social pressure. Complete autonomy is only achieved when an individual feels free from the need for approval of those surrounding him.

Turning to our general problem, we are attempting to establish certain correspondences between different series of facts. Personally I do not think it is possible to establish these correspondences between facts as such, but between *mechanisms of development*. It is the mechanisms of transformation which are our common denominators. Thus I should like to know more about just how these 'modes' you have described are transformed, how one becomes part of another or how one is replaced by another, in what way there is conflict between them. I think this is the crucial point for enabling us to find a common system for our future discussions.

LORENZ:

When I made the statement that the principle of grasping and ungrasping was applicable not only to overt oral movements but to locomotor and other movements of the striated musculature in general, I was only doing a thing which I always do with Freudian assertions: I first try to generalize them to the utmost extent—and then I try to reverse this process and 'ungeneralize' them. With Freud you always know that he is seeing something of extreme importance and you never know whether he has over-generalized or under-generalized, and quite usually he over-generalizes as I think is the case with the Oedipus complex, which develops in the way I have

already pointed out. Now in the case of sexual differences in behaviour, such as 'intrusiveness' and 'inclusiveness' and so forth, I find myself in the rare position of wanting to generalize something that Freud said farther than he did. What irks me about the diagram which Erikson has just shown us demonstrating these differences, is that it can be applied, with very little or no change, to animals which do not have, never have had, and never will have either a penis or a vagina. So the zone-theory certainly does not hold for these animals, but the principle of the diagram goes.

I want to point out to Eric modes of pair-formation, the mechanism by which partners of different sex are enabled to find each other. This was a real riddle with some cichlid species in which there is not the slightest sexual dimorphism either in bodily characters or in instinctive movements and in which, nevertheless, heterosexual pairs are formed with the same regularity as in organisms with a sexual dimorphism like that of a golden pheasant. When I set my pupil Beatrice Oehlert to tackle this problem I certainly did my best to handicap her with a prejudice: I was convinced that sex-recognition must somehow be effected by some kind of releaser characteristic of one of the sexes. Miss Oehlert's very thorough analysis of conflict behaviour brought to light an altogether different mechanism which, in my opinion, has a great deal to do with what Freud explained on the basis of the zone theory. If animals which, during the non-sexual period, are unsocial and territorial, are brought into contact with each other by the awakening of sexual drive, the unwonted proximity of a conspecific invariably activates, besides sexual responses, also those of escape and aggression. This 'conflict' is not, by any means, anything pathological, it is normal and necessary that all three drives are simultaneously activated. Indeed, the two fish would not approach each other, if ad-gression, in the literal sense of 'going there' did not bring them together.

Now you must realize that in these fish we really know to a considerable extent what movements are activated by which drive. We really know that this little erection of the dorsal fin or that spreading of the gill membrane means aggression, and that this slight sideways tilt of the body and folding of the median fins means activation of escape drive or that this little sideways twitch of the head denotes sexual excitation. Our knowledge is borne out by correct prediction of what the fish will do in the next second. In short, you may believe me that we really know what drive is at the moment contending with what other drive within one of these cichlids.

The important thing which Beatrice Oehlert found on this basis was the following: In both sexes, aggression, sexual drive and escape are being activated all the time, as long as both fish keep responding to each other. In both sexes, each of these three drives exerts a certain

inhibiting influence on the two others. But these inhibiting effects are not the same in the two sexes. In the male, aggression does not inhibit sexual behaviour, or inhibits it only very slightly. In other words, aggressive behaviour and sexual behaviour are perfectly mixable in the male; they can be superimposed on each other. But, in the male, escape behaviour effects an almost complete inhibition of sexual behaviour. In other words, if he is even very slightly afraid of the female, he is completely unable to respond sexually to her. In the female, it is the other way round. Beatrice Oehlert found that mixed forms of aggressive and sexual behaviour—which are so frequent in the male—do not occur in females. In the female, aggressive behaviour completely inhibits sexuality, even if sexual motivation is very strong. Quite on the contrary, females that are full of spawn, and stand badly in need of a male, are only all the more furious in their attack against any male which does not, in his turn, intimidate them. On the other hand, escape behaviour and sexual behaviour are compatible in the female.

Now this comparatively simple dimorphism in the inhibitory interactions between escape, aggression, and sexuality is—surprisingly—quite sufficient to explain the formation of heterosexual pairs in animals lacking any other form of sexual dimorphism. I am quite convinced that the same principle holds true for all those birds in which both sexes are potentially able to act either as male or female, dependent only on the social rank-relation to the partner.

Thus the parallel between the role played by male 'aggressiveness' in my example, and male 'intrusiveness' in Freud's theory is, I think, obvious. In other words I do not believe that males are intrusive because they have a penis, I believe that in vertebrates it is the males, and not the females, that have developed a penis, because ages before the first penis was ever evolved, males were more aggressive and the little mechanism of pair-formation, which Beatrice Oehlert discovered, was in full operation.

#### MEAD:

I think this illustration brings out beautifully the point that it isn't the penis and the vagina that create modes of behaviour; the penis and the vagina in human children and human adults are used as a form of communication. Communication among creatures differently equipped genetically would go on differently.

The things that the child has to learn as he matures are used as modes of communication, and one culture emphasises one thing and another another. One school of psychologists says that the trouble with anthropologists is that in one culture they will discuss orality and weaning as a clue and in the next culture they will discuss anal training. This is because we follow what is done in the culture itself

with its emphatic patterning of interrelationship to the child at a particular stage. In a culture which emphasizes the anal retentive mode, we may find insistence on the child getting a certain kind of oral training as part of anal training and discipline of movement. The emphasis on rhythm and on sitting in a certain way, moving in a certain way, defaecating at a certain time, eating a certain kind of food in a certain sort of way, may give a hypertrophy of maintenance function and an extraordinary impairment of initiative or operational functions as Grey Walter calls them. Manus culture twenty-five years ago heavily emphasized these control rhythmic functions at the expense of individual initiative and individual integration (Mead, 1956). You got this even reflected in the view of the self or the ego. Twenty-five years ago a Manus didn't have a whole personality. He had eight or nine names and each name was attached to the amount of property that had been given at his birth and people called him by one of these names in terms of their particular relationships to him. His emotions were located in different parts of his body, so that he thought with his neck, got angry with his belly, felt grief in his eyes and fright in his buttocks. And the society itself had no boundaries. It was a system of compensating pressures on the individual. An individual did things because other people did other things to him. There was no real centralized political idea of leadership. Now today, when they have come into the modern world, taken over the idea of being a little nation with boundaries, with membership in larger groups, with structure, with a fence around each village, with organized leadership, the individual now has something called a mind-soul, which is a single principle inside the individual directing people's relationships to each other and to a rather odd version of a Christian God. As this is developed the regulative inhibiting aspects of the toilet training and muscular training that existed twenty-five years ago have decreased and the emphasis on initiative has increased.

The extreme child-training in our own societies of thirty years ago was a perfect way of making a good accountant in a bank who took the same train every morning and who wanted to take the same train every morning and never made a mistake either. But it was a very poor way of making an individual who had more initiative and autonomy.

TANNER:

I have a specific question to ask Erik. In child development, is the location, so to speak, of the organ modes primarily in the brain or is it primarily peripheral? When you say the organs create the modes do you mean it in the literal peripheral-organ sense? There is a crucial question on this which should be able to be answered and

that is what do you know about the psychosexual development of pseudo-hermaphrodites? Their brains, as far as we know, are probably normally male- and female-developed, but their peripheral genital organization varies from case to case and is always more or less muddled up.

FREMONT-SMITH:

I can give a very short answer to this last question. I think the best data have been collected by Drs. Hampson, Money and Hampson (1956) under Dr. John Whitehorn at Johns Hopkins University. These investigators showed in quite a large group of pseudo-hermaphrodites of different forms of mal- or incomplete development of the genital organs that the mode followed by the child was determined not by the internal organ (whether it was testes or ovary) nor by the external organ (whether it appeared to be more male or female) nor by the presence of XX or XY chromosomes as determined in the skin, but by the social pressure, by the expectation of the family and the social group as to whether this little human object was a boy or girl. The family and social attitude was the most important one to be considered in planning a successful gender for the child.

LORENZ:

May I answer that question from another point of view? You know that the motor patterns of copulation are quite ambisexual in most mammals and that the females are able to execute the pelvic thrust exactly in such a way that it would effect introduction of the penis if they had one. If you watch the cows coming down from the Alps now they are slightly excited, and you can often see them performing male sexual movements, which shows that at least in them the behaviour pattern is built into the central nervous system.

TANNER:

Yes, I know. This is why I addressed my question to Erik, because what I am really trying to do is to pin down to what extent these descriptions in psychoanalytic language of the erotogenic zones and the stages of development are symbolic only and to what extent they are quite concrete.

ERIKSON:

I would say they are both; they *must* be both in the human. The abortive phallic thrust-movements which occur in the cow are, indeed, mode fragments, lacking only the proper executive organ; but they are not comparable to the mode-fragments in the human which become integrated only by becoming symbolic at the same time as they become real: i.e. they become social, aesthetic and moral

modalities at the same time as they become an expression of the organism. At any rate, the modes and modalities have a highly symbolic quality from the beginning. The highly affective tone in the parents' warnings and approvals give the child the feeling that an event is a significant reality, has a high symbolic value; and I would think it is both the affectivity in the child's specific drivenness at a particular stage *and* the affectivity of the guiding adults which give it that value. This is probably the very reason why full sexual consummation is delayed so long in the human—so that society has the opportunity to bring together drive-energy, instinct-fragment, and symbol-communication in such a way that important values are once and for all transmitted.<sup>1</sup>

ZAZZO:

I think we should stress the point that at no age in his life does the child have an equilibrium of his own, an autonomous equilibrium such as an animal would have; but that the child's equilibrium always necessitates adult intervention and this intervention is variable from one civilization to another and gives to each age its own particular aspects. I think we tend too much to study childhood as a psychogenesis through mechanisms within the child without considering that at each stage, each moment, the adult intervenes and that without the adult there is no human child.

ERIKSON:

I entirely agree. Certainly it is one of the human problems that an inner regulation is accomplished only by a gradual internalization of an outer regulation first presented by the maternal person, then by the parental persons, by the basic family and so on. I also think the superego is nothing else but the beginning of adulthood in the child. It is the taking over of the moral self-regulation. You can often see when the child's superego is developing by the way he treats younger children; he treats younger children as if he were already an adult. That often looks like a caricature; and we know the tremendous burden which spurts of precocious adulthood can be on the child's equilibrium. Whatever the ego is, its dominant area of equilibration at the threshold of adulthood can only be psychosocial. This is why we have to protect children for so long, why we have to educate them for so long, and why we owe them a whole functioning society—and not just a good mother.

Yet, many adult egos and superegos, joined in a society, must come to a relative cultural equilibrium with one another, in order to

<sup>1</sup> It is important to note that, in this discussion, I am employing the more general use of the concept symbol, and do not refer to the specific case of 'Freudian' symbols.

create that prolonged state of protection which we call childhood. But such a cultural equilibration, necessary as it is for the survival of each individual, is at some times and in some ways antagonistic to the individual's equilibrium. We would like to simplify this matter in claiming, for example, that a 'sane society' makes sane people, but it probably is not that simple. You probably can't have a society without its own forms of 'insane' tensions, just as you can't have individuals without conflicts. It is in the nature of social equilibrium that there must be people who 'pay' for the society's equilibrium—just as individuals 'pay' for their personal equilibrium with some sacrifices of wishes, capacities, opportunities.

## FIFTH DISCUSSION

### General System Theory and the Behavioural Sciences

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BERTALANFFY:

You will have seen that my memorandum (Part I) offers for consideration in this Group a certain theoretical structure called General System Theory; and I want now to discuss the application of this structure to problems in the behavioural sciences.

Before doing so, I would like to make an introductory remark about the use as well as the limitations of models in science. A model never *is* the thing concerned; it only represents certain *aspects* in a more or less adequate way. To be aware of this is of primary importance. There is a very general tendency for symbols and models to be taken for the things of which they represent certain aspects only. The outcome of this tendency is what we may call the fallacy of the nothing-but. If we fall into the fallacy of the nothing-but, we have propositions such as—all phenomena in the world are nothing but the mechanical play of atoms; the reaction of an animal is nothing but an aggregate of reflexes; the brain is nothing but an electric calculator; the psyche is nothing but id, ego, and superego; and so forth *ad infinitum*. All such propositions are quite valid in so far as they are taken as a description of certain aspects of reality, but they are preposterous and easily refuted if the model is taken for a metaphysical entity. All scientific theories are constructs, models or, if you will, mythologies. Consequently, they must be remodelled and improved with increasing knowledge and must never be taken for granted and never taken as absolute or as an expression of ultimate reality.

One of these models is what is called General System Theory. Perhaps the simplest way to introduce you to this is to tell you how I myself came to the idea. If we survey the development of modern science, we realize that very similar general viewpoints and concepts have appeared in independent fields, often without the research worker in one field knowing of the work done in the other. For example, problems of organization, of wholeness, and of dynamic

interaction are the most urgent ones in modern physics, chemistry, physical chemistry and technology. In biology, in contrast to the analytical way of thinking and experimenting, that is, isolating components such as chemical compounds, cells, reflexes, or whatever the case may be, problems of an organismic sort appear everywhere, and the question again is about wholeness, organization, and dynamic interaction. We have a parallel development in psychology, starting with Gestalt psychology and manifest in many other modern trends. And if we look at the social sciences, again it is problems of wholeness, interaction, organization, etc., which appear to be most important and pressing.

There is a second and even more interesting aspect. We find not only a parallelism in the general development of science, but quite often isomorphic laws in fields which have little or nothing to do with each other. For example, in the theory of populations of animal or plant species, certain systems of equations have been developed by Lotka, Volterra and others for the so-called ecological equilibrium, the struggle for existence and so forth. Later on, it turned out that the same sort of differential equations may be used in problems of chemical kinetics. The entities compared are totally different, but it appears that the general conceptual model, an ecology, so to speak, on the one hand of animal species, and on the other hand of molecules, is essentially the same. The origin of such correspondences is obviously the fact that isomorphic models can be and actually are applied in quite different fields.

So I came to postulate a new discipline which would deal with these matters, and which is called General System Theory. This theory deals with models and laws applicable not to one particular system, but to systems in general.

The argument leading to such generalized theory runs something like this. If we look at physics, we find that it investigates systems of various levels of generality: from very special systems as they are used by the engineer who builds a bridge or an automobile, up to laws such as those of thermodynamics, which apply to systems of a very general nature, whether they are caloric, mechanical, chemical or whatever else. However, there seems to be no limit or border-line. Why not ask for principles and laws holding for systems in general, not only for physical systems but also for systems in the biological, behavioural and social fields? And actually it turned out that it was possible to elaborate such a theory of generalized systems.

I think the first time I talked about this idea was at the University of Chicago in 1938. Then came the war, and I also was afraid of what Gauss, the mathematician, called the *Geschrei der Boeotier*. I feared that my reputation as an experimentalist would be damaged if my preoccupation with such highly theoretical matters were

uncovered! So nothing was published until after the war. Then, however, I soon found that this idea was not a personal idiosyncrasy or fancy, but rather was one expression of a trend which was present in various quarters. I found that quite a number of scientists in different fields had thought on essentially similar lines. Some even used the same mathematical model I had used, drawing from it, however, rather different deductions. This was, for example, the case with Ross Ashby. In addition, there were other new developments, such as information theory, cybernetics, game theory, decision theory, and operational research which used mathematical models which were different, but had a similar general interdisciplinary aim. So General System Theory is an expression of a rather general trend, prevailing in various fields of modern science.

GREY WALTER:

You speak of 'General System Theory' as a theory of theories. I'd like to ask: do you use the word 'model' as being essentially synonymous with 'theory'?

BERTALANFFY:

Yes. I don't clearly see the difference between 'theory' and 'conceptual model'—the latter being the more fashionable term. What is essential is the hypothetico-deductive character of a construct so that consequences can be derived from it.

GREY WALTER:

But don't you think it is worth distinguishing between hypotheses in three forms: a verbal form, using language; a mathematical form, which of course is a language but a very special form of language with rules of its own; and a working model form, which is neither linguistic nor mathematical and which has limitations and advantages of its own? For example Ashby, whom you mentioned, differs from you in that he made a machine before he wrote his book, and he therefore discovered in making and working and observing his own machine where he was wrong. For me, and for many other people who are thinking on convergent lines, the value of a working model is that it is a 'crystallized hypothesis'. It is absolutely clear, it is quite brittle, and when it breaks, it breaks with a loud bang. There's no question of it being bent, almost imperceptibly, to fit new facts. We know words can change their meaning almost imperceptibly. Mathematics bends less easily and there are many brilliant mathematical biologists who have promoted generalizing theories, but nobody I know understands them. They use eccentric, idiosyncratic algebras—even Ashby, who has tried very hard to systematize his thinking in his recent book *Introduction to Cybernetics* (1956), is

hard to understand for those of us who have not had formal and prolonged mathematical training. I should have thought that the word 'model' should usually be kept for those hypothetical propositions which are in the literal sense mechanical. They are not equivocal as words are, and they are intelligible as mathematical expressions usually are not. If we use 'model' interchangeably with 'theory' then I think we're liable to condemn theory because it has the limitations of models, and condemn models for having the limitations of theory.

BERTALANFFY:

I think our difference is largely semantic. We can distinguish material models and conceptual models. Material models are your tortoises, or Ashby's homeostat, or Lillie's iron wire. Conceptual models are theoretical constructs. I most heartily agree with what I suppose was in your mind, namely that conceptual models may be so general that you can do little with them. I would not draw an absolute border-line between verbal and mathematical models however; mathematics is only a highly formalized form of language and thinking.

GREY WALTER:

Well, yes. But I think there is a very important distinction between ordinary vernacular and mathematical language. The rules of mathematics are invariable, whereas the virtue of what we are saying now is that it is essentially poetic, it can be misunderstood, it means different things to different people. Mathematics is unambiguous—there is no noise in it. It is an absolutely noise-free channel, which is a special case of communication.

BERTALANFFY:

This discussion is related to the question Konrad Lorenz raised in his memorandum (Part I); whether with such general conceptions, there is danger of throwing away the specific and essential character of the phenomenon concerned. The answer is emphatically 'yes'. This danger is a very real one, but it is present everywhere. We do this all the time, only we must be careful to be aware of doing it. For example, take the system of mechanics, which is certainly a legitimate science. It tells you that Newton's apple and the planets and the tides follow the same law—the law of gravitation. Now this is all right as far as it goes; but it does not mean that apples and planets and the ocean are all the same. Or to put it a little differently, there are innumerable other aspects of apples and planets which you just don't handle with this particular kind of model. It was the pitfall of the so-called mechanistic view of the world that it took

mechanics as the all-embracing model of all phenomena in the world.

LORENZ:

I am always very strongly conscious that all our experience, all we know of the world, is also a model. As the pictures are modelled in the grain of our retina, so everything is modelled in the grain of our central nervous system. That is of course a truism, but I want to call attention to it because this fact makes it so difficult for us to remember that our consciously made models are models of a model.

GREY WALTER:

A model of a model is a model.

LORENZ:

A model of a model *is* a model, of course. But that's why the two aspects must be kept apart in our thinking, because there is only a difference of degree between them.

BERTALANFFY:

Our world picture is a model conditioned on the one hand by biological factors, meaning our organization as a primate with certain characteristics of sensory apparatus, a certain organization of the nervous system, etc., and, on the other hand, by cultural and linguistic factors. What aspects we pick out depends on biological as well as cultural factors. In scientific thinking, however, there is the reverse tendency, which I have called progressive deanthropomorphization (Bertalanffy, 1953b). By this I mean that the elements which are specific to our biological organization and to our cultural and linguistic bias are progressively thrown out of the scientific world picture. What eventually remains is a conceptual system—in the best case a mathematical system—which does represent certain aspects of reality, whatever this means, even though certainly quite different modes of description and quite different aspects would be possible. In this becoming free from our biological and cultural biases lies, I believe, the dignity of science.

Coming back to General System Theory, we can also look at the problem in a somewhat different manner. Most of you will know a remarkable paper by Warren Weaver, the founder of information theory, on Science and Complexity. Weaver (1951) gives a brilliant exposé of one basic problem which is involved here, the line of argument being something like this: Classical physics or, to speak in a more general way, the mechanistic approach, has led to an elucidation of processes where one-way causal chains can be isolated. The classical approach was also very successful with respect to what

Weaver calls laws of unorganized complexity, that is, where the behaviour of a whole can be considered as the statistical result of practically innumerable elementary events. These are laws of disorder which, in the last resort, stem from the second principle of thermodynamics. But then, we have also posed to us the problem of *organized* complexity, that is, of laws of interaction within a finite number of elements. It is here that the classical approach lets us down. You can rephrase this by saying that the great problem is what are the laws of organization. These are very difficult to state; remember, for example, that even the three-body problem in mechanics is insoluble, or solvable only by way of approximation; or think of the difficulties appearing in simultaneous differential equations when the condition of linearity is abandoned. This problem of organized complexity appears particularly in fields like the biological, behavioural and social sciences. I think it is reasonable to say that all these modern approaches, general system theory, cybernetics, game theory, etc., are different attempts to tackle this problem.

Thus the main functions of general system theory and of the related approaches just mentioned would be essentially two: (i) to develop a superstructure of science applicable in different fields and providing a basis for the unity of science; and (ii) to provide conceptual models, principles and laws in those sciences which at present lack them—that is particularly the biological, behavioural and social sciences.

General System Theory does not claim any monopoly, and the only claim it makes is that for certain aspects or for certain kinds of problems it seems to be useful, while with respect to other aspects and problems, approaches like that of information theory, cybernetics, or game theory, etc., might be better. Even so, there are certain indications that further unification of these approaches may eventually be possible. For example, if you take cybernetics and the feedback model, it will probably turn out as a special case of a more general model of dynamic interaction (Bertalanffy, 1951a). Again, information is defined as negative entropy in information theory, but negative entropy also plays an important role in the thermodynamics of open systems. Perhaps there will be found a way to translate one approach into the other, comparable to the translation between quantum statistics and wave-mechanics which are different mathematical descriptions but give the same results. However, in the present stage it will be best to follow up one model and see how far we can get.

Quite a number of applications of General System Theory have been made (Society for the Advancement of General Systems Theory, 1956). However, for this presentation, I would like to enumerate only a few consequences of possible interest to this group.

General system theory permits the exact definition of many notions

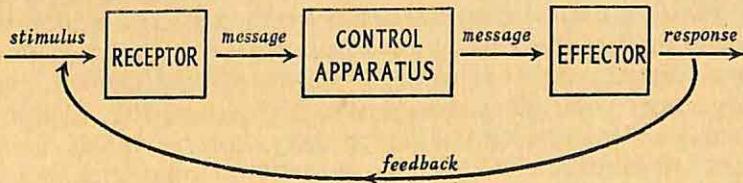
which are alien to physics but are indispensable in the behavioural, biological and social sciences (Bertalanffy, 1950a; Ashby, 1952; Miller, 1955). This applies to concepts such as hierarchical order, differentiation, centralization, control, the whole and its parts, and teleology in its various forms. General system theory is able to give exact definitions of such concepts in mathematical terms which, so to speak, strip them of the metaphysical or vitalistic character which was often associated with them. In the social sciences for example Narroll and I tried to give a quantitative measure of social organization in terms of the numbers of craft specialties in a society versus population, using the principle of allometry (Narroll & Bertalanffy, 1956).

I am sure I have much agreement here when I say that the notion of teleology, while taboo in the mechanistic world view, is now taken seriously; we are quite well able to give concrete models for different aspects of teleological behaviour. Some material models of this sort were presented by Grey Walter; other models are equifinality, trial and error by step functions following Ashby, etc. I think it is important to point out that notions of this kind are not taken to be vitalistic or metaphysical any more, but are considered amenable to scientific thinking and research.

It seems to me that General System Theory offers a better model of behaviour than previous theory did. It appears that in what may be called the classical theory of behaviour, three aspects have been fundamental.

(a) The first is linear or one-way causality as expressed in the classical reflex-arc and stimulus-response scheme, and also in the feedback scheme of homeostasis (Fig. 5). Even though the latter assumes a circular process, one-way causality remains unchanged.

FIG. 5  
FEEDBACK SCHEME



(b) The second characteristic is the consideration of all behaviour from an economic or utilitarian viewpoint, that is, as serving the maintenance of an equilibrium or biological survival. This is essentially the concept of homeostasis.

(c) The third is the viewpoint of the primary reactivity of the psychobiological organism, as expressed in the stimulus-response

scheme. The organism is considered essentially as a penny-in-the-slot machine, set into action by external stimuli.

I have already mentioned some criticisms of this classical scheme in my precirculated paper, and Konrad Lorenz has advanced similar ideas. Thus, it can be said that the classical model is inadequate because: (i) behaviour as a whole precedes linear-causal or reflex reactions, as can be abundantly shown ontogenetically as well as phylogenetically: ontogenetically, if you think of the observations on early foetuses by Coghill and others; phylogenetically if you consider the behaviour of some lower animal like a jellyfish where there is behaviour as a whole but no definite one-way reflexes (Bertalanffy, 1952). (ii) Consideration of all behaviour as homeostatic, in the nature of 'coping with reality', a 'defence mechanism' and the like overlooks those activities which fall under the category of play, creativity, and so on. (iii) Primary activity ontogenetically and phylogenetically precedes stimulus-response and reactive behaviour.

The homeostasis model which is fashionable nowadays still adheres to the classical viewpoints mentioned, one-way causality, economic consideration of behaviour, and primary reactivity of the organism. In contrast, general system theory provides the background for a new concept of the organism and of behaviour, emphasizing action-as-a-whole as primary, allowing for non-utilitarian activities, and taking into account that intrinsic activity generally precedes reactive behaviour.

In criticism of the notion of homeostasis I would like specifically to emphasize two points. The closest German translation of this term is *Regelmechanismen* which is an appropriate word, as it expresses that 'mechanisms' are under consideration. Now mechanisms of the Cannon or homeostasis type of course are present in many regulative phenomena of the organism, from the regulation of body temperature to that of pH, blood sugar, and many others. But these appear to be secondary apparatuses. The primary regulability of the organism is due to dynamic interaction within a steady state. Superimposed on this are secondary regulative mechanisms of the homeostatic type.

My second comment is concerned with those activities which are variously called play exploration, creativity, etc. I would take exception to a distinction made by Franz Alexander between maintenance (or homeostatic) activities and activities of surplus energy, the latter including propagation, play and creative activities, etc. It seems to me difficult to distinguish between them. For example, you have a rat or mouse, and it is running around in its cage. This would be surplus activity, as the animal doesn't need to do it, and could just sit down and eat chow from the container. On the other hand, in the natural environment the rat must run around to explore and to find

the necessary amount of food, so it is really a maintenance activity. Or a young colt is hopping around in the corral for no particular reason, seeming to be a playful animal. But such play activities do have a survival value, hence are 'homeostatic' mechanisms. It seems difficult to draw a borderline between immanent maintenance activity and surplus activity.

LORENZ:

I could not disagree less with what you have just said about the impossibility of finding surplus activities. You would find that in these species in which surplus activity seems to abound, the whole system of action of the species was built up on the presence of surplus activity. All these animals with a huge differentiation of exploratory behaviour *live* by the exploratory behaviour. What I call the specialists in non-specialization like rats, like *Corridae* in birds, like man, are the most tremendous successes biologically that you can imagine; and if you ask why, the ecological answer is very simple.

Let me take the raven as an example: the raven can live in the desert, leading the life of a vulture, and it can live on bird-islands leading the life of a skua, and so on and so forth because in its youth it treats everything as if it were biologically important. If you see a raven dealing with an unknown object, you will find that its overt behaviour consists successively of (a) escape reaction, (b) attack reactions, (c) feeding responses, and (d) the very special response of hiding. It first treats the object as a potential enemy by being very careful, then attacks it from a certain angle—from behind, if it has a head, which is an IRM for front end and rear end—and ends up by performing all the hiding activities, by taking this thing, tearing it to pieces and hiding it in clefts. Now you might think that the bird wants to eat that thing, but this is not the case. This activity is so preponderant that in a state of exploratory behaviour, you cannot lure away the raven by the best baits, by the most delicate morsels even if it's hungry. Anthropomorphically and functionally speaking, this raven does not want to eat this thing, but it wants to know whether it is eatable *in theory*. This is research, you see, and the rat as well as the raven, and man also, lives by virtue of exploratory behaviour. That is their main adaptation. The animal lacking in many specific adaptations has one particular adaptation and that is exploratory behaviour—and it's certainly not a surplus activity. Your rat running about or your lion running up and down a cage only seem to do surplus things if you keep thinking on the basis of reflex theory. . . .

BERTALANFFY:

That's what I call the 'error of the cage'.

**LORENZ:**

That's the error of the cage, exactly! And the moment you know about automatic activities, it is perfectly clear that this poor animal must somehow get rid of the endogenous impulses it is producing all the time. So I couldn't disagree with you less.

**FREMONT-SMITH:**

Could one say that the homeostatic mechanism is superimposed on the primary activity of the organism, but superimposed in order to free the organism to exhibit its primary activity in a wide variety of environments—to return to Claude Bernard again?

**BERTALANFFY:**

Certainly. It is a general principle that specialization permits higher efficiency and entails loss of regulability. You have a primitive society—everybody is his own tailor, farmer, engineer, doctor and what have you, and everybody can do everything, but on a pretty low level. Then you come to a highly specialized society. Here doctors are supposed to be better and tailors definitely are and farmers also. On the other hand, the specialists become irreplaceable and if a major catastrophe occurs, for example, the engineers of the water-works of London or New York go out of business, this means absolute disaster. Something similar applies to the specialization of the biological organism. Specialized homeostatic mechanisms do set the organism free of changes in the outer world, allowing it to devote its activities in a broader range of environment, and at a higher level.

**GREY WALTER:**

I make a distinction between the administrative and the operational aspects of organic life, which is similar to Alexander's, but the distinction is, in my mind, a *vectorial* distinction, not a scalar one. The essence of the administrative or homeostatic functions, their diagnostic character, is that the feedback is a negative feedback. It is an error-operated system of the most primitive type, a reflex; it is very easy to imitate and as I mentioned in my memorandum is to be found in most well-regulated households in the water-closet—which came even before the thermostat.

Now on the other hand the operational aspects of life are those which have a *positive* feedback and are therefore essentially unstable. This is a vectorial distinction; it is the sign and not just the scale that is distinctive. By experiment it is sometimes possible to move the vector around from negative to positive or vice versa and thus to show that in certain cases, but not in others, a homeostatic or administrative function or mechanism which mediates stability can be converted into an operational or unstabilizing one.

The reason I think this distinction is important is that it enables one to design experiments in terms of the vector or sign of the information flow, without recourse to such rather vague notions as autonomy or energy. I think you should be able to design experiments to find out whether an animal running around freely is behaving homeostatically or operationally. For example, if the application of a stress induces torpor or indifference or sleep, that would suggest that the effect involves increasing negative feedback; if there is increased irritability and instability the vector is becoming positive. This is where I should expect signs of personality to appear, particularly in higher mammals. To return for a moment to our terms of reference, human children may respond to stress by withdrawal and apparent indifference or by aggression and agitation; it may be worth considering whether in the first case there is aggravation of a homeostatic process and in the second evocation of an operational one. Furthermore there may be critical phases in development when one or the other mechanism is more likely to appear under stress.

#### BERTALANFFY:

What Cannon meant by homeostasis is indicated by examples like the regulation of body temperature which represents a feedback circuit like that shown in Fig. 4. I would criticize the use of the word 'homeostasis' for designating adaptation in general, as is often done, because regulation on the basis of dynamic interaction seems to be the primary phenomenon, and mechanized and structure-bound feedback regulations appear to be superimposed as a secondary layer. As a classical example, take regulations in early embryonic development. A normal sea-urchin ovum yields a normal larva. You divide the ovum in two halves, and each half becomes a complete larva. You bring two eggs to fusion—again you get a normal larva. This was Driesch's so-called first proof of vitalism. However, we can explain this, at least in general terms, within the theory of open systems as an example of equifinality. But certainly you cannot imagine feedback mechanisms being involved because there is a dynamic interaction of practically an infinite number of parts. I am critical of using the term 'homeostasis' as synonymous with adaptation because this inflates the notion so that it may lose its original meaning.

#### ERIKSON:

I want to say one word about the pleasure I feel in hearing all this because it gives, as it were, the theoretical underpinning for something which clinically has impressed me in the last few years. This theory of the surplus in play, of course, goes back to the idea that those animals play most that do not have to take care of their

own food supply and of their own security because either their mothers take care of it or humans do. The origin of all these theories lies in the economic doctrines of the last century, the influence of which on Freud has been vastly underestimated. You find theories in the classical economics of the last century which already speak of humans in the aggregate as pleasure-and-pain machines, in terms of gains and losses. I personally would say that nothing has done more harm in psychoanalysis and in its application than the *Weltanschauung*, one might almost say the ethics, which grew out of this 'economic' trend of thinking. Thus, the ego has been treated in psychoanalysis as a kind of merchandising agency which bargains for pleasure and self-expression. By this, I do not mean that Freud's psycho-economics is not fruitful; but it is the anthropomorphic equation of man with one of his functions, which has made him 'nothing but' a storehouse of inner bargains.

Speaking practically, we at Riggs have taken quite a number of chances with patients during the last few years, and found out that patients were able to do things which, according to our theories, they were unable to do. We found that we were inclined to keep them ill by imposing on them interpretations concerning an inner economic determinism, telling them *why* they couldn't do what they couldn't do, without realizing that they were in a state of latent activity—where something active is going on which is merely waiting for a chance to meet the world of opportunities. The combination of interpretation and encouragement of specific activity brings about an entirely different inner economic state. We have recently had experiences where the patients have made our life difficult by suddenly wanting to do things which patients are not supposed to be able to do and we had to change the structure of our institution and the relationship of the institution to the community. Only an integration of our ego-theories with such theories of functioning as Piaget advances can get us closer to the nature of this problem.

#### BERTALANFFY:

What Erik has just said gives me an excellent start for my concluding remarks. Psychobiologically it is customary to consider all behaviour, and particularly the behaviour of the child, as 'coping with reality', as 'defence mechanisms' and the like. Now, yesterday, instead of going to Montreux as I was advised to do, I spent the time preparing the present talk. But certainly our charming Chairman did not force me to do so and so far as I am able to see I have put it together, not as a homeostatic device for maintaining my disturbed mental equilibrium or as a defence reaction against this group, or as an outflow of my Oedipus complex, but mostly for the fun of it.

ERIKSON:

The last two things are not mutually exclusive!

BERTALANFFY:

No, I agree. But what I want to emphasize is that the psycho-biological organism is characterized by primary activity. Reflex mechanisms crystallize, so to speak, out of this primary activity in the way of progressive mechanization.

LORENZ:

Someone once said that the amoeba is less of a machine than a horse is!

BERTALANFFY:

That is exactly what I wanted to say.

One, though by no means the only, aspect of general system theory is its consideration of *open systems*. This is an important aspect in so far as it has led to original developments in physics, physical chemistry and biophysics. Conventional physics by definition deals only with closed systems, that is, systems which do not exchange matter with the environment. So physical chemistry, in kinetics, tells about the reactions, their rates and the chemical equilibria eventually established in a closed reaction vessel where a number of reactants are brought together. In a similar way, conventional thermodynamics expressly declares that its laws apply to closed systems: according to the first principle, in a closed system energy is constant; according to the second, in a closed system entropy must increase. This implies that eventually the process comes to a stop at a state of maximum entropy. As you know, the second principle can be formulated in different ways. One of these is to say that entropy is a measure of probability, hence a closed system tends towards a state of most probable distribution. This, however, is a state of complete disorder. If you have, for example, red and blue beads, then it's very improbable that all red beads should be on one side and all blue beads on the other side of a container; the most probable state is that all the beads are mixed up. Something similar applies to molecules having different velocities: it is improbable to have all fast molecules (or a high temperature) on one side, and all slow molecules (or a low temperature) on the other; the most probable state is to have them evenly distributed over the space, resulting in a state of thermic equilibrium.

However, we find systems in nature which by their very definition are not closed. The most important, though by no means the only, example is the living organism. The living organism is an open system where there is import and export, continuous building up

and breaking down of material. So we are confronted with a rather embarrassing situation, namely that the laws of conventional physics, by definition, do not apply to the living organism, or, speaking more precisely, apply only to limited aspects and processes of it. For example, the transport of oxygen from the lungs to the tissues is based upon the establishment of chemical equilibria between reduced haemoglobin, oxyhaemoglobin and oxygen. We can apply the conventional principles of kinetics in this particular case because this is a fast reaction which comes to an equilibrium in a short time. However, we cannot apply the familiar formulations of kinetics and thermodynamics to the organism as a whole because these are slow processes which don't go to equilibrium but rather are maintained in a steady state.

So, in order to arrive at a true biophysics, a physics of the living organism, we have to generalize the principles of physics to include open systems. Such a theory leads to a large number of consequences which in our limited time obviously I cannot discuss. I want to clarify only one point: namely, that the theory of open systems is not a clever *aperçu* or a mere programme of things to be done, but is a well-developed theory, by no means consummate but progressing rather rapidly at the present time. The generalization of physical chemistry, of kinetics and thermodynamics, has been made and applied to very different problems. I may mention that the so-called irreversible thermodynamics, of which the thermodynamics of open systems is a part, is a major development in modern physics and has led to novel developments and solutions in thermo-electricity, thermo-osmosis, electro-osmosis, thermo-diffusion, the so-called 'fountain effect' of helium II, and many other problems which do not belong to the present discussion.

The applications of the theory of open systems in biology and biophysics include, for example, the kinetics of open systems in isotope experiments. The theory of the transmission of excitation in the nerve, as developed by Hill and Rashevsky, is a special case of the theory of open systems. So is the theory of sensory and, in particular, visual perception, as developed by Hecht, and the theory of growth, as developed by myself. The phenomena of overshoot and false starts which are not found in conventional chemical systems going towards equilibrium, but are seen in many physiological phenomena, can also be interpreted in terms of the theory of open systems. Such theory also allows calculation of the energy requirements for maintenance of the steady state of the organism.

The theory also leads to consequences of a general character. One is the so-called principle of *equifinality*. Briefly, this means the following. In a closed system the final state is unequivocally determined by the initial conditions. For example, when you have a

chemical equilibrium then the final concentrations eventually established by the reactants will depend on the initial concentrations, namely, what you have put in in the beginning. In contrast to this, equifinality is a characteristic of open systems; if and when an open system approaches a steady state this will be the same, irrespective of the initial conditions and the course taken by the process in approaching the steady state. This can be easily shown mathematically; if the kinetic equations of an open system are solved for the steady state, the parameters of the initial conditions drop out and the steady state is defined only by the system-constants, the parameters of reaction and transport.

It may be noted that this equifinal behaviour was considered to be the foremost 'proof of vitalism'. As already mentioned, a normal and complete larva develops, for example, from a normal ovum of the sea urchin, from each half of an experimentally divided ovum, or from the fusion of two ova. This proves, according to Driesch, that biological regulation cannot be explained in terms of physics, but only by the action of a vitalistic 'entelechy' directing the processes in anticipation of the goal. It is interesting to note that Driesch's argument is quite correct so far as conventional physics goes; for in the physics of closed systems, equifinal behaviour is not found. However, the so-called proof of vitalism evaporates when you take into account the physics of open systems.

There is a second problem which constitutes another allegedly vitalistic feature of the living world: the question of entropy and the living organism. The problem can be formulated in the following way. There appears to be a sharp contrast between inanimate and animate nature, or, as it was sometimes stated, a violent contradiction between Kelvin's degradation and Darwin's evolution, between the law of dissipation in physics and the law of evolution in biology. According to the second principle of thermodynamics, the general trend of physical events is towards states of maximum disorder and levelling down of differences with the so-called heat death of the universe as the final outlook when all energy is dissipated into heat of low temperature and the world process comes to a stop. In contrast, the living organism is a system of extreme improbability and highest degree of order which nevertheless is kept fairly constant in so far as we are considering the adult stage. Furthermore, we find, in embryonic development and in phylogenetic evolution, a transition toward higher order, heterogeneity and organization. The ovum, in ontogenesis, advances from a seemingly undifferentiated state to the extremely complicated structure of the developed organism. In evolution we find a transition from the lower to higher and highest organisms.

In fact, this problem is an easy one, for the apparent contradiction

between entropy and evolution disappears within the generalized thermodynamics of open systems. Perhaps I should make this point a little more technical.

There is a modern development called Irreversible Thermodynamics in contrast to classical or conventional thermodynamics. The equations of classical thermodynamics are applicable only to closed systems. It can be said that the classical doctrine should rather be called 'thermostatics'—as is frequently done at present. Conventional thermodynamics or thermostatics proves to be insufficient in so far as states of nonequilibrium, processes of transport in and out of the system, and irreversible processes come into play.

The modern generalization of kinetics and thermodynamics is a remarkable development which was partly stimulated by biological considerations. It remains a pleasing thought to me that I was one of the first to pose the problem and to propose the concept of the 'open system' from the biological viewpoint in 1934. Only much later, I learned that a Belgian, Defay, had spoken of the thermodynamics of open systems in 1929. I developed some basic principles of the kinetics of open systems in 1940, and a similar approach was made almost simultaneously by the Canadian biophysicist, Burton (1939). Irreversible thermodynamics was started in Germany by J. Meixner, and its development is particularly due to the Belgian school of thermodynamics, Onsager, Prigogine, de Donder, de Groot, and others.

Irreversible thermodynamics is characterized by the fact that it introduces a number of principles which are novel compared with classical thermodynamics. Essentially they are the following: (1) the generalization of the entropy function which I am going to discuss in more detail in a moment; (2) the so-called phenomenological laws; (3) the Onsager reciprocity relations; and (4) the principle of microscopic reversibility. From these principles, irreversible thermodynamics as a generalization of classical thermodynamics can be derived, as reviewed in the books by de Groot (1951) and Denbigh (1951). This is not the last possible generalization and further work is in progress of which I would like to mention that of Reik (1953) which seems to be the most general formulation of thermodynamics hitherto achieved.

On the basis of the theory of open systems the apparent contradiction between entropy and evolution disappears. In all irreversible processes entropy must increase. Therefore the change of entropy in a closed system is always positive and order is continually destroyed. In open systems, however, we not only have production of entropy due to irreversible processes but we also have import of entropy which may well be negative as in the case of the living organism which imports complex molecules high in free energy.

The basic principle in the classical thermodynamics of closed systems is that entropy must increase:

$$dS > 0$$

which is the simplest expression for the second principle. However, a generalized entropy function was introduced by Prigogine which also covers open systems and reads:

$$dS = d_e S + d_i S$$

This equation states that the change of entropy in an open system is composed of two parts, namely the change of entropy by transport  $d_e S$ , and the entropy production within the system,  $d_i S$ , due to irreversible processes such as chemical reactions, diffusion, heat conduction, etc. Now according to the second principle,  $d_i S$  must always be positive. However,  $d_e S$ , meaning entropy transport, may be positive, zero, or negative. It is negative if material rich in free energy is introduced into the system. Consequently, the total change of entropy,  $dS$ , can, according to the amount and the sign of  $d_e S$ , be either positive or negative, and there is no contradiction between the second law and the thermodynamics of open systems. Open systems, and in particular the living organism, can avoid the increase in entropy and maintain a thermodynamically improbable state; or they may even advance toward states of decreased entropy or increased organization.

LORENZ:

I take it this means that life increases entropy to the Universe.

BERTALANFFY:

Yes. What you choose as an open system or as a closed system depends on what you want to investigate.

LORENZ:

Considering the Universe as a closed system, because there isn't anything else, life lives at the cost of the entropy increase of the Universe.

BERTALANFFY:

Yes, this is true.

PIAGET:

As we are here for a synthesis and to make a certain number of interdisciplinary transitions I should like to transfer to a problem which interests me very deeply and which is one of the aspects of my essay. If you translate the ideas of thermodynamic entropy into

ideas of entropy relative to information, the problem life-and-entropy which you have just dealt with appears again in the following form: are logical structures going to correspond to an entropy maximum or, on the contrary, to an anti-entropy process, a sort of antichance or Maxwell's demon before all the corrections which have been introduced into this idea since Szilard; or on the other hand must another solution be investigated, a tertium? Well, at the moment I have the impression, without having had time to think it over, that your solution for the biological field corresponds to this tertium which we are seeking in the logical field. In the cognitive field also there is exchange because there is an open system without this being opposed to increase in entropy. It would thus be a question of seeing how one could transpose the terms of your solution into terms of information and logic. We could admit that logical operations are the organ of exchange, whereas the total content of these exchanges would necessarily obey the laws of chance.

**GREY WALTER:**

I was going to say exactly the same thing as Piaget. In many approaches to this problem there has been a temptation to compare the integral equations describing entropy and those describing information, which look very much the same, and to consider information as negative entropy: I wonder if one might say that logical reasoning is a closed system in which information can never increase, in fact is bound to diminish.

But what I called in my commentary on Piaget's paper 'statistical reflexion', is in a sense an open system in which information may actually increase. If this is so then there may be a corresponding loss of information elsewhere, just as entropy is increasing in the Universe as a whole, if we assume a simple naive view of the Universe. If we may actually reduce the rate of increase of entropy in statistical reflexion (of which I suggested logical reasoning in a special case just as closed systems are a special case of open systems) then one may actually get a paradoxical gain of information, which has always puzzled people and gives one an excuse for some sort of vitalistic notion of creative mentality.

**PIAGET:**

I am not at all sure that logical reasoning is a closed system. I should like to have Bertalanffy's opinion.

**BERTALANFFY:**

It certainly appears that in some way the thermodynamics of open systems and information theory have to be brought together, even though at present we don't know exactly how this is to be done (cf.

Quastler, 1954). In this regard, I would like to mention one point which is often overlooked. Information theory is essentially concerned with possible decisions, that is, probabilities. In its present form, it is not concerned with what is sometimes called semantic information. To put the problem in the simplest way: a royal flush in poker is called a highly 'improbable' event, and in fact it requires a considerable number of 'bits'. But by itself, a royal flush is not more improbable than any odd combination of cards, a ten of spades, plus the five of hearts plus the queen of diamonds, etc. The royal flush is 'improbable' only if we lump together the probabilities of all other combinations against one singular case. But why the flush is a singular case is not defined in terms of decisions or bits of information. This problem of 'semantic information' naturally appears also in other applications of information theory and information theory in its present form is not capable of defining why a certain state is singular as compared to others.

**PIAGET:**

I agree with practically everything Bertalanffy has said, except on one point which is very general, and which no doubt depends more on wide conceptions and interpretations than on data which can be verified by experiment.

Bertalanffy tells us that two systems exist in an organism. There is a primary system which is a total of dynamic interactions within a stable state and then there are secondary mechanisms, that is to say homeostases, which occur later as regulations in the primary mechanism. It is this duality which worries me. I do not see any decisive reason for introducing duality instead of admitting a steady state open system with regulations straight away.

In fact this primary mechanism has two characteristics: it is steady and it is open. Well then, if it is open, in order for it to be steady there must already be regulations. If not, and precisely because it is open, it is exposed to all the alterations due to external perturbations and will stop being steady. It follows that there are regulatory mechanisms from the beginning, mechanisms which have not been added but which are inherent. In other words, this system of dynamic interactions already contains what in my language I call equilibrium and what Bertalanffy in his language calls stability. In short, I myself would see unity from the start where Bertalanffy sees duality.

This brings me to a second question: the position of logic. Grey Walter tells us in his paper in Part I that logic is a sort of closed system which teaches us nothing, which is a collection of redundancies, and that the important reality is constituted by the statistical system of information within which logic becomes crystallized.

I have a remark to make here which brings us back to unity. I

think we must carefully distinguish in our discussion between the logic of the logician and that of the individual, and the only thing which is of interest for the aims of our group is the logic of the individual. The logic of the logician, of course, *is* a closed system because it is an axiomatization made after the event and made so that it constitutes precisely a closed system. This logic does not teach us anything, I agree: it is only an instrument of control and not of information or increasing information. But if we turn to the logic of the individual—the logic which develops in the child and which results in a state of equilibrium in the adolescent and the adult and of which the logician makes an axiomatic system after the event—the logic of the individual is above all an operatory system. Now an operatory system constitutes a group of activities which are in equilibrium in the sense in which I have used the term, that is to say in a sense which is both very special because it is the only state where one really achieves equilibrium and at the same time very general, because the system of operations in equilibrium is in fact only a special case of a vast whole, that of regulations and homeostases, etc. Just as a short time ago I was upholding the fact that there was unity where Bertalanffy introduces duality, I think equally that in the problem of relations between logic and information there is complete unity between logic and the acquisition of information. Logic is not the result of the putting together of information, it is a structuration and organization, which is what permits exchange between the individual and the external world. It is the total sum of the regulations which permit this bringing in of new information, this bringing of external entropy into Bertalanffy's equation.

#### BERTALANFFY:

It was certainly not my intention to establish a dualism between dynamic regulations on the one hand and fixed machine-like regulations, feedback and the like, on the other. If I created such an impression it is only due to the necessary brevity of my remarks. You may have noticed that I have spoken of a 'progressive mechanization' which implies that there is no dualism of regulations but a gradual transition. I had best quote from a paper where I discussed this question in detail:

'... There is no sharp border-line between "dynamical systems" and "machines" . . . The construction of a "machine" essentially means that, within a system of forces, conditions of constraint are introduced, so that the degree of freedom in certain causal chains is restricted, usually so that only one course is possible, and causality runs one way. But even in man-made machines, we have all transitions from fixed to loose coupling, allowing for only one, or for several degrees of freedom. But this is precisely what we mean and what

we find in nature. The primary and basic regulations seem such as in systems with a minimum of constraint and therefore a high degree of freedom. Ontogenetically and phylogenetically, conditions of constraint evolve, making, on the one hand, the system more efficient, but limiting, on the other, dynamic interaction, degree of freedom, and regulation after disturbance. . . .

'In this sense, feedback represents an important, but special type of system behaviour. "Dynamics" is the broader theory since we can come, from general system principles, always to regulations by machines, introducing conditions of constraint, but not *vice versa*.' (Bertalanffy, 1951a, p. 360.)

Lastly I want to emphasize that, speaking of 'systems', we must distinguish two different kinds: one is the 'material' or 'natural' system, the other the 'conceptual system' such as mathematics and logic. The term 'system' is thoroughly misleading if this is not borne in mind. Naturally what I have said about closed and open systems refers to material or natural systems. A conceptual system like geometry or logic—that's quite another story.

FREMONT-SMITH:

And alas, at least for the present, we have no next instalment for it to be continued in. But my idea of a study group of this sort is one that generates a discussion which ends open-ended, and I think that this is a very good end to open on, or open to end on.

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psychosexual development in children and a discussion led by von Bertalanffy on General Systems theory and the relation of this to Piaget's concepts.

It is unlikely that so distinguished a group of people from so many disciplines and from so many countries has ever before been brought together for informal discussion in the field of human biology. The verbatim transcript has by careful editing been condensed to make about one-third of its original length and to present the material in a more readable form, but the spontaneity of informal discussion in a small group has been retained. This series, of which the present volume is the fourth and last, will be found to be of the greatest interest and value to all those concerned with the genesis of the adult human personality, whether their prime concern is with physical or psychological or sociological factors.

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